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Karl Pearson: 1910

VOLUME XXIX FEBRUARY, 1938 PARTS III AND IV

KARL PEARSON

An Appreciation of some Aspects of his Life and Work

By E. S. PEARSON

Part II: 1906–1936*

1906–1911

The year 1906 was a dividing point in Pearson's life; that he felt it so himself there is much evidence to be found in his letters. The thirteen years since 1893 had been marked by a growing personal friendship and scientific collaboration between Weldon and himself which could never be replaced. Together they had wrestled with the development of a new mathematical technique and had shown with abundant illustration how necessary was its application in many of the fundamental problems of biology. They had met fierce and sometimes unscrupulous opposition and had faced it together, Weldon with his dashing cavalry charges into the foe, Pearson with his heavier artillery. Since the former had moved to Oxford, there had been a continual exchange of correspondence between them on the problems with which each was concerned at the moment; letters full of the excitement of some new discovery or of frank criticism of each other's ideas. Of Pearson's letters to Weldon only a short series written about 1900 seems to have been preserved, but these throw so much light on the place of that friendship in the history of biometry that I shall quote two of them now, even if they belong strictly to an earlier period†.

* The first part of this memoir appeared in *Biometrika*, XXVIII (1936), pp. 193—257. The two parts will shortly be re-issued together in book form.

† This group of letters only came into my possession after the publication of the first half of this memoir.

7, *Well Road,*
Hampstead, N.W.
April 23, 1900.

My dear Weldon,

I got home to-day to find your letter. I am awfully sick at getting back into this loathsome Town and harness, when there is so much to be seen and observed in the country. A competent man tells me that the Yaffle is the green woodpecker and that the peasants never call anything but the lesser or spotted woodpecker, a woodpecker. Here it is intensely hot and nothing but house-painters in view down the backs of houses. At Gatwick I could watch from my work-table two snipe preparing to nest, two pairs of plovers ditto, all in a marshy bit of common at the bottom of our small garden. Also a wryneck in a box within an inch of my window. The owner encourages birds by sticking up pots and boxes for them, and the pools on the common abound in water birds I don't know even the names of. Now I must come and sit here until July when it will be too late to see anything. What brutes those Oxford Electors were to condemn me to endless years of London!*

I am afraid the poppies are giving you endless trouble. I shall not get across to Highgate to look at my own sowings till this week end. But I shall hear something from Oliver, Tansley and Macdonell of theirs. They are all far less elaborate though than yours! I will send you as soon as I can the averages of the selected poppies.

I have written to 11 persons for 10 house sparrow nests. Suppose 50 per cent come up to the scratch I shall get 50 nests of 5 eggs or more apiece. I don't think I can get by bothering everybody I know more than 70. Can you provide 30? Latter of Charterhouse who is going to try and do ten suggested that museums often have a number of clutches of eggs of each species. What are your longest series at Oxford? How many plovers have you? Plover give too few eggs but by taking more individual nests one might make it up.

Yours always sincerely,
K. Pearson.

7, *Well Road.*
May 3rd, 1901.

My dear Weldon,

I am too sleepy to write much, so here are a string of statements:

(i) I have been nigh to Gloucester today and found rooms on the Cotswolds near Bisley. I think by biking to Fairford it ought to be possible to get to Oxford. This is something off one's mind. I hasten to tell you in case you should be asking your friends about places for us, and wasting your precious time.

(ii) The correlation of barometric height between Bodø, the northernmost Norwegian Station, and Valentia is hardly sensible, but between Bodø and Funchal it is sensibly *negative*!

(iii) The value of least girth to the breadth has, on the average of 700 house sparrow eggs, the value of 3·15. Do you think this a reasonable approximation to the value of π?

* This is a reference to Pearson's unsuccessful application for the Savilian and Sedleian Chairs at Oxford in 1897 and 1899 respectively; see Part I of this memoir, p. 224.

(iv) If you will send me particulars as to what you want done re Italian statistics, I think I can set somebody on to it.

(v) We have found out that the interval between births and the difference between duration of life of elder and younger brothers or sisters are sensibly correlated. The elder lives as a rule 4 years longer. In other words *order of birth* influences longevity. I have suggested in this final paper that much of the variability one finds in an array of brethren will be found like longevity to be correlated with order of birth. Is not this akin to differences in early and late flowers?

(vi) Have you read Guarta (?) on crossing of white and waltzing mice? I have only seen Davenport's account of it, but it seems as if the percentages were based on too few cases to be of real value. Still it appears to resemble Mendel's case of pea hybrids.

(vii) It is not for mathematical formulae, but to give large correlation tables, that the large size of *Biometrika* is worth fighting for. I think we must be assured that the Press don't intend to take a big publisher profit off us!

Yours always sincerely,

K. P.

Please mention to the Press that we shall want articles in *foreign tongues*.

The inspiration and encouragement gained from personal contact, largely during those holiday meetings in the country, had been great too. "You have hardly realised and I don't think *he* did," Pearson wrote in April 1906 to Mrs Weldon, "how mentally refreshing it was to me being near him for a few weeks and how it sent me back fit for work with new vigour and new ideas. He always gave me courage and hope to go on...."

In the years to follow Pearson stood alone, the leader of a cause, who must plan its action and fight its battles. It is true that for five years more he gained much from an ever growing friendship with Francis Galton, but neither the counsel of an old man nor the help of a body of younger followers could replace the comradeship that was lost. Perhaps more than at any moment in his life, in this black year of 1906, he needed to draw on that fund of courage which he possessed.

While it is easy to place too much emphasis on a classification of a life's work into periods, it is I think justifiable to associate the period 1906–1914 with Pearson's foundation of a research institute where the ideas and methods, thrown up in rapid succession during the previous years of excitement and discovery and dealing with investigations as yet regarded almost as hobbies, could be developed under more secure conditions into an established branch of science. It seems well therefore to preface this section of the memoir with some account of the origin of the Biometric and Eugenics Laboratories, which were later, in 1911, to form Pearson's Department of Applied Statistics, and to make clear how he regarded their relationship and their purpose.

With the steadily increasing application in biology of quantitative methods of comparison it is likely that the term biometry may come to be used in a wider or different sense than that originally understood. It is therefore of some interest

to record what Pearson, writing in 1920, regarded as the function of his Biometric Laboratory*.

"The origin of the Biometric Laboratory must be sought in the year 1895, when the present Galton Professor gave his first course on the mathematical theory of statistics—probably the first course given on the modern mathematical theory at all—to two or three postgraduate students, one of whom is now Reader of Statistics in the University of Cambridge. From that year the statistical course became annual, and as the field of this form of investigation had been very little worked, a school sprung up which has since been recognised as the 'Biometric School,' and the group of workers, occupying a small room at University College, later termed the Biometric Laboratory, issued a long series of memoirs, which formed the basis of the English school of mathematical statistics. The object of this school was to make statistics a branch of applied mathematics with a technique and nomenclature of its own, to train statisticians as men of science, to extend, discard or justify the meagre processes of the older school of political and social statisticians, and in general to convert statistics in this country from being the playing field of *dilettanti* and controversialists into a serious branch of science, which no man could attempt to use effectively without adequate training, any more than he could attempt to use the differential calculus, being ignorant of mathematics. This task was a very arduous one, for statistics in one form or another are fundamental in nearly every branch of science in precisely the same manner as mathematics are fundamental in astronomy and physics. Inadequate and even erroneous processes in medicine, in anthropology, in craniometry, in psychology, in criminology, in biology, in sociology, had to be criticised, not for the pleasure of controversy, but with the aim of providing those sciences with a new and stronger technique. The battle has lasted for nearly twenty years, but there are many signs now that the old hostility is over and the new methods are being everywhere accepted."

It is clear from this statement that Pearson regarded his Biometric Laboratory as essentially a centre for training postgraduate workers in a new branch of exact science and for the application of the methods learnt, partly as illustrations of technique, in a variety of different directions. The field of application for which the technique had been devised was that of biology, and for this reason the word *βίος* had been associated with *μέτρον*, but Pearson had already illustrated the use of the new methods in meteorology and astronomy. Writing to Galton in 1908 ((18) III A, p. 333) he said:

"I have so much in hand that to close one phase of my work only means more progress in other phases. I should only feel sad if something were to happen which closed *all* phases of my work. Why, if Eugenics and even Biometry were closed down, I should turn to Astronomy with all my energy and time; I know how badly statistical knowledge is needed for problems therein!"

In his anxiety to leave Galton free to change the control and organisation of the Eugenics Laboratory, Pearson may have passed over too lightly in this letter

* The quotation is taken from a printed statement entitled *History of the Biometric and Galton Laboratories*, drawn up in 1920 in connection with the opening of the new building given by Sir Herbert H. Bartlett to house the Department of Applied Statistics.

the attraction which the biological sciences had for him; but I think it is true to say that however much he was fascinated in these earlier years by research into the theory of evolution and later on by the study of man, that "queen of the sciences," it was at bottom the statistical method of approach and the application of mathematical tools to the analysis of observational data which he felt it to be his main purpose to advance. The spirit of the biometric school could be described in Galton's words: "Until the phenomena of any branch of knowledge have been submitted to measurement and number it cannot assume the status and dignity of a science." Pearson may at times have been over confident of the strength of his tools, but his purpose was to demonstrate the essential need for their use in many fields. When, for example, he wrote to Weldon that one long piece of research into errors of observation* was "intended as a torpedo for the astronomical ark," the launching of this missile was in no factious spirit; no doubt, as he wrote this phrase, there was a twinkle in his eye, but beneath there was a deep conviction that the methods of mathematical statistics opened a new road for scientific investigation.

In 1904 Galton had made a gift of £1500 to the University of London for the furtherance during three years of the scientific study of Eugenics. As a result, his Eugenics Record Office had been started in rooms, first at 50 and later at 88, Gower Street provided by University College, with a staff consisting of Mr Edgar Schuster as Research Fellow and later of Miss Ethel M. Elderton as his assistant. One of the first pieces of work undertaken was the compilation of a register of Able Families. The direction of the Office was entirely in Galton's hands.

Towards the end of 1906 Schuster wished to resign his appointment in order to undertake more purely biological work, and Galton, who was now 84 years old and at the time unwell, felt that the task of choosing a successor and planning a research programme was too heavy for him to undertake. He therefore decided to hand over the control of the Office to Pearson so that it might be run in contact with the Biometric Laboratory. Correspondence regarding the transfer and the objectives of what was henceforward to be termed the Francis Galton Eugenics Laboratory is set out fully in *The Life, Letters and Labours of Francis Galton* ((18) III A, pp. 296—307). David Heron, who had been attached to the Biometric Laboratory since 1905 and who for ten years was to be Pearson's leading statistical colleague, became Galton Fellow, Miss Elderton became Galton Scholar and Miss Amy Barrington, part-time Computer. The Laboratory was not transferred into rooms in the College itself until October 1907.

Pearson was somewhat hesitant in taking over this new responsibility. Besides the additional work that it threw on his shoulders, he was aware that his views on eugenic research, involving a patient collection and reduction of data, did not correspond exactly with those of Galton, who was eager for quick results and pleased with slighter contributions that would catch the public imagination. But he realised

* This interesting and perhaps little noticed piece of work was based on experiments carried out by Pearson, Lee, Yule and Macdonell between 1896 and 1900. It was published in 1901 under the title "On the Mathematical Theory of Errors of Judgment, with Special Reference to the Personal Equation" (45).

that unless he stepped into the breach this pioneer effort, which held so much promise for the future, might fail from lack of a directing hand.

And so at the beginning of 1907 we find Pearson head of the Department of Applied Mathematics, in charge of the Drawing Office for engineering students, giving evening classes in Astronomy, the director of two research laboratories and the editor of their various series of publications and of *Biometrika*. It was indeed a tremendous task which only a man possessing his power of concentration and his faculty of rapid shifting of mind from one subject to another could hope to have carried out successfully. Fortunately for his health, as one of his assistants of those days writes, "London weather saw to it that many astronomy nights must be cancelled."

It is impossible to refer here in detail to all the research work which Pearson initiated in the next few years. Apart from what was published in *Biometrika*, it was issued in three main memoir series, (i) the *Biometric Series* and (ii) the *Studies in National Deterioration*, both issued as *Drapers' Company Research Memoirs*, and (iii) the *Eugenics Laboratory Memoirs*. The *Biometric Series* contained, to start with, further papers of the series "Mathematical Contributions to the Theory of Evolution," (43) 1904, (44) 1905, (46) 1906, (47) 1907, and (48) 1912, those long memoirs containing mathematical theory and biological applications that the Royal Society had shown unwillingness to publish. Later it contained the three memoirs on Albinism ((49) 1911 and 1913), two on the "Long Bones of the English Skeleton" ((50) 1917 and 1919), and one on the "Sesamoids of the Knee Joint" ((52) 1922), which was a reprint of *Biometrika* articles (51). The allocation of papers between the National Deterioration and the Eugenics Laboratory Series was probably determined by the Laboratory to which the author was attached and the funds used for publication, as much as by the subject-matter of the paper. Thus, the four memoirs on the statistics of Pulmonary Tuberculosis ((53) 1907, (54) 1908, (55) 1910 and (56) 1913) were published in the former series and the four on Alcoholism ((57), (58), (59) 1910 and (60) 1911) in the latter.

From the point of view of subject-matter we may usefully classify the most important of the publications of these years under three heads: (i) memoirs concerned with the collection and analysis of fundamental data regarding inheritance; (ii) memoirs in which statistical methods were used in an endeavour to throw light on important social and eugenic problems of the time, and which often involved the Laboratories in prolonged controversy; (iii) contributions mainly concerned with statistical theory.

(i) *Collection and analysis of fundamental data regarding inheritance*

Foremost under this heading comes the *Treasury of Human Inheritance*, of which Parts I and II of Volume I were published in 1909 (61). It was planned on a comprehensive scale, intended to provide data in the form of pedigrees, illustrative plates and verbal descriptions for the measurement of all phases of human heredity.

"The publication of family histories," Pearson wrote in his Preface, "—whether they concern physique, abnormality, ability or achievement—whether they be new or old—is the purpose of this *Treasury*. Students of heredity find great difficulty in obtaining easy access to material bearing on human inheritance. The published material is voluminous, scattered over a wide and often very inaccessible journalistic area. The already collected although unpublished material is probably as copious but no central organ for its rapid publication in a standardised form exists at present. The Eugenics Laboratory alone possesses several hundred pedigrees of family characteristics and diseases which it is desirable to make readily accessible. Many medical men possess similar material....

"A complete pedigree is often a work of great labour, and in its finished form is frequently a real work of art. To the many who have felt the delights of genealogical inquiry, we would say: Widen your outlook, recognise that there is something beyond names, births and deaths worthy of record, and, as it is harder to ascertain, more exciting in the pursuit. The pedigree of temperament, disease, ability, and physique which ought to replace the old nominal pedigree—if not for exhibition—at least in the family archives, is the true measure of the fitness of a stock, and the best guide to the younger members in their choice of career and alliance.

"For a publication of this kind to be successful at the present time, it should, as I have indicated above, be entirely free from controversial matter. The *Treasury of Human Inheritance* therefore contains no reference to theoretical opinions. It gives in a standardised form the pedigree of each stock."

The collection of the material was made possible through extensive collaboration with the medical profession. Some ten contributors, of whom one of the most important was William Bulloch of the London Hospital, were responsible for different sections of Volume I; the general editing and standardisation of the work was undertaken in the Eugenics Laboratory. The standard was a high one and it is easy to see Pearson's influence running throughout its 550 pages, in the care for detail, the clearness of arrangement and the striking photographic illustrations. That decision not to use the data to illustrate any theory of inheritance, but to aim at an absolutely unbiased gathering, sifting and publication of material has made the first and succeeding volumes of the *Treasury*, as its Editor had hoped, a record of great and lasting value.

There are several references to albinism in the early volumes of *Biometrika*. Both Darbishire and Schuster at Oxford had carried out at Weldon's suggestion certain experimental crossings of different races of mice, to determine how far albinism could be regarded as a Mendelian unit-character. In 1904 Weldon had contributed a note on "Albinism in Sicily and Mendel's Laws*," which led to some discussion with Bateson on the interpretation of the data. It was no doubt in order to get to the bottom of some of the questions in dispute by obtaining a much larger supply of reliable data, that Pearson in collaboration with two ophthalmologists, Edward Nettleship and C. H. Usher, commenced about 1906 to collect the material that was later published in the three volumes entitled *A Monograph on Albinism*

* *Biometrika*, III, p. 107.

in Man ((49) 1911 and 1913). The scope of this inquiry went far beyond the testing of this or that theory of inheritance. It aimed, as in the case of the *Treasury*, at putting on record a wealth of data on the subject collected from published sources and by new inquiry; combined with this was research into the character of pigmentation in the eye, the hair and the skin for both man and certain animals. The headings of the eleven chapters contained in Parts I and II provide some idea of the range which was covered: Introductory; Early Notices of the Occurrence of Albinism; Geographical Distribution of Albinism; The Albinotic Skin (Historical and Theoretical); Leucoderma; Partial Albinism; The Albinotic Eye (Man); Albinotic Hair (Man and Lower Animals); The Albinotic Eye (Lower Animals); On the Seasonal Variation of Winter White Animals; Experimental Breeding in Dogs with Reference to Albinism and Piebaldism.

In the historical chapter, as in the section on Dwarfs in the *Treasury*, Pearson's early gift for historical research found free play. Part of the secret of his immense power for creative work lay in this variety of his interests. He could turn with enjoyment and profit from algebra and arithmetic to piece together that tradition of an albino race placed sometimes in Africa, sometimes in India, sometimes in South America, which has turned up from time to time from the days of Pliny and Ptolemy; or to collect early records of albinotic or piebald negroes brought into this country as slaves. The volumes were beautifully illustrated by pedigrees, photographs, coloured plates of samples of hair and microscopic drawings of eye and hair sections, etc.

The physiological investigation made quite clear the complexity of the problem; it did not seem possible to class an individual as an albino or a non-albino, for the degree of pigmentation might vary enormously. Besides this, some portions of the body might be devoid of pigment and not others.

"Albinism is not in our opinion," the authors wrote in the introductory chapter, "a single narrowly-defined condition, which exists or does not exist in an individual. The frequency of the individual sub-classes, and the degree of intensity even within these sub-classes, are points which require very careful consideration; it is only comparatively recently that trained observers have turned their attention to the collection of these cases of incomplete and imperfect albinism." ((49) p. 9.)

The final discussion of the material, with chapters on the vital statistics of albinism in man and the relation of albinism to other pathological states, as well as the final reduction of the statistics of heredity of albinism in man were to be issued in a fourth volume*. This has never been published though much of the material is available among Pearson's papers; it was no doubt the war, intervening in 1914, that took Pearson from a subject to which he never found time to return.

An interesting line of investigation, that had its origin in the research into albinism, was the experimental breeding of dogs. From a foundation stock of three albino Pekinese, Jack, Jill and Tong, acquired by Nettleship in 1908, some sixty

* This volume was to be called Part III of the *Albinism*; the bibliography, pedigree plates and description of pedigrees were published in 1913 as Part IV.

albino puppies had been bred by 1913. Certain of these were crossed with black Pomeranians to produce a hybrid Pompek. The object of the inquiry was to study both inheritance of coat colour and of head shape, in which the two stocks showed a fundamental difference. Four measurements of the head were taken at two or three different ages on all puppies, and skeletons and skins of certain typical animals were preserved.

A preliminary report of the experiment was published in 1913 in Part II of the *Albinism*, but the breeding was continued until the stock was finally dispersed in 1933 on Pearson's retirement*. The analysis of the very long series of results was another piece of unfinished work that he had hoped to complete when freed from College duties.

One purpose of the provisional report of 1913 was to raise two searching questions. Is it possible to explain the results of an experiment such as this by the simple Mendelian rules of dominance and segregation? If not, what are we to make of the too ready conclusions of social and eugenic reformers whose minds seem to be carried away by the fascination of a single simplified law of inheritance? Such, I think, is the substance of the questions raised in the following paragraphs:

"Of course it may be asserted that these indices [obtained from the head measurements] are very complex characters, and may be compounded of many Mendelian units. To this we must reply that albinism is of a precisely similar character, there is a very large series of characters involved in the pigmentation of different parts of the eye, the skin, the hair and the internal organs, and complete albinism of the one does not involve that of the others. Length of coat is very much of the same character, for there is an immense variety of lengths of hair on head, back, tail and legs, which vary from breed to breed. In our Pekinese the colour of the coat is of a similar character, hairs of widely different tints are found on the same dog, not only in different parts but often in the same parts, and occasionally different parts of the same hair are quite differently pigmented. If it be justifiable to use 'Jewishness' and 'Gentileness' of face as contrasted Mendelian units, one recessive to the other—notwithstanding the innumerable factors which combine to give facial expression,—we are, we hold, justified in investigating whether our relatively simple indices do or do not 'mendelise.'" ((49) pp. 483—484.)

"The problem of whether philosophical Darwinism is to disappear before a theory which provides nothing but a shuffling of old unit characters varied by the appearance of an unexplained 'fit of mutation' is not the only point at issue in breeding experiments. There is a still graver matter that we face, when we adduce evidence that all characters do not follow Mendelian rules. Mendelism is being applied wholly prematurely to anthropological and social problems in order to deduce rules as to disease and pathological states which have serious social bearing. Thus we are told that mental defect,—a wide term which covers more grades even than human albinism,—is a 'unit character' and obeys Mendelian rules; and again on the basis of Mendelian theory it is asserted that both normal and abnormal members of insane stocks may without risk to future offspring

* A further paper by K. Pearson and C. H. Usher on "Albinism in Dogs" was published in 1929 (62).

marry members of healthy stocks*. Surely, if science is to be a real help to man in assisting him in a conscious evolution, we must at least avoid spanning the crevasses in our knowledge by such snow-bridges of theory. A careful record of facts will last for ages, but theory is ever in the making or the unmaking, a mere fashion which describes more or less effectually our experience. To extrapolate from theory beyond experience in nine cases out of ten leads to failure, even to disaster when it touches social problems. In all that relates to the evolution of man and to the problems of race betterment, it is wiser to admit our present limitations than to force our data into Mendelian theory and on the basis of such rules propound sweeping racial theories and inculcate definite rules for social conduct. Even if the offspring of an albino parent be themselves normal, we cannot advise them that all is safe if they marry into normal stock; for not only is Mendelism as yet undemonstrated for human albinism, but who shall determine what is 'normal' stock, when over and over again the albino appears in the mating of two stocks which have no record of previous albinism?—Let us rather adopt the tone of the soothsayer in *Antony and Cleopatra* and when we are asked 'Is't you, Sir, that know things?' reply modestly 'In Nature's infinite book of secrecy a *little* we can read.' We await the gradual building up of more complete knowledge." ((49) p. 491.)

Now, some twenty-five years after these lines were written, perhaps the best tribute that could be paid to the time, money, energy and almost affectionate care which Pearson bestowed on the breeding of his dogs through so many years would be the reduction and interpretation of these collected data in the light of the best genetic knowledge of our day. The battle between Biometry and Mendelism is surely over.

(ii) *Research and controversy*

We must now consider a few of the publications falling under the second of the headings given on p. 166 above: memoirs in which statistical methods were used in an endeavour to throw light on important social and eugenic problems of the time. One of the most important of the inquiries of this type, to which seven of the Laboratory publications were devoted between 1907 and 1913†, was concerned with the statistics of Pulmonary Tuberculosis. In these years a great deal of money was being collected and spent in Great Britain on what was termed the Fight against Tuberculosis. With the discovery of the tubercle bacillus by Koch, the idea that infection was the determining factor held the field; popular imagination was directed towards a fight to destroy the bacillus and the conditions which were supposed to encourage its existence; advanced and infectious cases were to be isolated in sanatoria to prevent the spread of the disease; while members of tubercular stocks were told they might safely marry provided they lived with a good supply of fresh air.

Largely because no adequate data were available, the campaign was not based on any reasoned examination of figures; much of it was an appeal from the "market

* C. B. Davenport, *Heredity and Eugenics*, University of Chicago Press (1912), p. 286.

† While the present section is headed 1906–1911, it has been necessary to take some latitude in the discussion of papers; the work in most of those referred to in the following paragraphs was *initiated* before 1911 though the publication date may have sometimes been later.

place" of just that kind which might be expected to rouse K. P. in his "study." His first paper, based partly on an analysis of data from the Crossley Sanatorium, Frodsham, was published in 1907 (53). This was followed by a statistical analysis by E. G. Pope of data from the Adirondack Sanatorium in America, which Pearson completed for the press in 1908 after Pope's death (54); a year later there appeared an inquiry by Charles Goring based on the family history of 1500 criminals (63).

The main conclusions to which this work seemed to point may be summarised as follows: (i) The predisposition to tuberculosis—the tubercular diathesis—was inherited at much the same rate as other physical characters in man. (ii) The existence of a much higher correlation for tubercular diathesis between parent and child than between husband and wife was a strong argument in favour of the hereditary factor; on the pure infection theory it would be expected that husband and wife would be at least as likely to infect one another as parent would be to infect child. (iii) The correlation of diathesis between husband and wife existed, but varied from one class to another, being highest in the more educated classes. This correlation was of the same order as that found between husband and wife for a number of physical and psychical characters; it had already been described as the correlation due to assortative mating, measuring the tendency of like to marry like. In the middle classes this coefficient for tubercular diathesis seemed to be almost the same as for insanity. Tubercular stocks possess certain mental characteristics and it was conceivable that members of them might be to some extent sympathetic to each other, so that there was an actual sexual selection of those likely to become tuberculous. In the same way eccentric and mentally ill-balanced stocks may have an attraction for each other.

These papers were followed in 1910 and 1913 by two joint memoirs from W. P. Elderton and S. J. Perry, (55) and (56), who carried out an actuarial investigation into the comparative mortality rates (i) of the general population, and of tuberculous patients (ii) who were, and (iii) who were not, treated in sanatoria. They were forced to conclude that there was no clear evidence of a lower mortality among the second than among the third of these classes, although it was very difficult to obtain adequate comparable material. Further, they could find no evidence to support the claims of the advocates of the tuberculin treatment.

Pearson gave an admirably clear popular account of the meaning of all these investigations in a lecture delivered at University College in March 1912, afterwards published in the *Eugenics Laboratory Lecture Series* (64). A more vigorously critical attack against dogmatic assertions by some members of the medical profession, and in particular against Newsholme's *The Prevention of Tuberculosis*, was published in 1911 in the pamphlet "The Fight against Tuberculosis and the Death-rate from Phthisis" (65), issued in *Questions of the Day and of the Fray*, a series devoted to the discussion of the more controversial topics of the hour.

If at times the controversy over tuberculosis was hot, that over the question of alcoholism raged far more fiercely. The first contribution to the subject from the

Eugenics Laboratory was published by Ethel M. Elderton and Karl Pearson in 1910 (57). Their object was to investigate from series of data from Edinburgh and Manchester whether there was evidence that the alcoholism of parents had any marked influence on the mentality and physique of the offspring as *children*; they were not at the moment concerned whether these children when adult might be likely to exhibit alcoholic or other unsatisfactory tendencies, since on this point the data used could provide no information. The difficulties of the problem and the limited scope of the investigation were set out with much care and clarity; the final conclusions may be quoted:

"To sum up then, no *marked* relation has been found between the intelligence, physique or disease of the offspring and parental alcoholism in any of the categories investigated. On the whole the balance turns as often in favour of the alcoholic as the non-alcoholic parentage. It is needless to say that we do not attribute this to the alcohol but to certain physical and possibly mental characters which appear to be associated with the tendency to alcohol. Other categories when investigated may give a different result, but we confess that our experience as to the influence of environment has now been so considerable, that we hardly believe large correlations are likely to occur.

"If, as we think, the danger of alcoholic parentage lies chiefly in the direct and cross-hereditary factors of which it is the outward or somatic mark, the problem of those who are fighting alcoholism is one with the fundamental problem of eugenics. We fear it will be long before the temperance reformer takes this to heart. He is fighting a great and in many respects a good fight, and in war all is held fair, even to a show of unjustifiable statistics. Yet the time is approaching when real knowledge must take the place of energetic but untrained philanthropy in dictating the lines of feasible social reform. We can only hope that this intrusion into the field of alcoholic inquiry will be recognised as an earnest attempt to measure the true influences of a grave social evil. Yet we have our fears,...*."

The paper was a well-written and unbiased scientific contribution, although no doubt there was a certain challenge in the concluding paragraph and its quotation. Its publication stirred up, however, a veritable hornets' nest of critics. Cambridge economists and medical men who had written on the subject of alcohol joined with platform orators of various temperance organisations in an excited buzz of criticism and misinterpretation of the memoir. Its authors were accused of every scientific blunder and almost of social and moral delinquency. Once the battle was joined Pearson hit back with characteristic vigour. The first pamphlet of *Questions of the Day and of the Fray* series issued in 1910 (66) contained an answer to the economists who had criticised the memoir on the grounds that the populations dealt with were not fair samples of the working-class population; later in 1910 Pearson and Elderton published a joint reply to their medical critics in the *Eugenics Laboratory*

* The paper ends with a quotation from Plato's *Euthyphro* in which Socrates says (in Jowett's translation): "For a man may be thought wise; but the Athenians, I suspect, do not trouble themselves about him until he begins to impart his wisdom to others; and then for some reason or other, perhaps, as you say, from jealousy, they are angry."

Memoirs (58); and in 1911 Pearson answered further criticisms of Sir Victor Horsley and Dr Mary Sturge in another *Day and Fray* pamphlet (57).

"Dove si grida, non é vera scientia" was one of Pearson's mottoes. His line of reply was to take the statistics of various earlier writers with which his critics had attempted to confound him and to show that while in some cases they were most unreliable, in others, when properly analysed, they pointed to conclusions hardly differing from those which he and Miss Elderton had reached. It was almost too easy a task. "We have not discussed at length," the authors wrote at the conclusion of their joint reply, "all the data provided by Sir Victor Horsley and his colleagues; we have merely sampled their material to indicate how little real knowledge flows from their methods of treatment. But if occasion arises we shall go further; our illustrations are not selected, they are a random sample of the 'rebutting' evidence produced by the medical critics of our memoir."

This controversy also brought the Eugenics Laboratory into conflict with certain leading members of the newly formed Eugenics Education Society. It was a conflict which Pearson would, if possible, have avoided, partly because he knew that it pained Galton, the Founder of the first and the Honorary President of the second organisation; partly because he realised that it did no good to the reputation of Eugenics as a science. In a Foreword to the first issue of the *Eugenics Review* Galton had written: "There are two sorts of workers in every department of knowledge—those who establish a firm foundation, and those who build upon the foundation so established." Pearson doubted from the start whether these two types of workers could in fact co-operate, but he had hoped it would be possible for them to follow their own lines, leaving each other alone. The following letter, already published in *The Life, Letters and Labours of Francis Galton* ((18) III A, pp. 371—372) expresses his views clearly:

Hampstead
February 7, 1909.

My dear Francis Galton,

Thank you most heartily for your very sympathetic letter. I agree so wholly with what you say—there is need for the purely scientific research, and for propaganda. I feel that the former demands two essentials: we have got to convince not only London University but the other universities (i) that Eugenics is a Science and that our research work is of the highest type and as reliable and sober as any piece of physiological or chemical work, (ii) that we are running no hobby and have no end in view but the truth. If these things can be carried out we shall have founded a science to which statesmen and social reformers can appeal for marshalled facts. If our youthful efforts were mixed up in any way with the work of Havelock Ellis, Slaughter or Saleeby, we should kill all chance of founding Eugenics as an academic discipline. Please don't think I am narrow, or that I do not admit that these men have done or may do good work. All I say is that I could not get the help we are getting from the medical profession, from pathologists or physiologists, if we were supposed to be specially linked up with these names. Rightly or wrongly it would kill Eugenics as an academic study. All I want is to stand apart doing our scientific work, not in any way hostile to the Eugenics

Education Society, giving it any facts we can or an occasional lecture, but not being specially linked to it in any manner. For this reason I am rather sorry that X. has gone on to its Council, because it makes a link, which I think it is better for Laboratory and Society not to forge—it will hamper the freedom of both. My policy, however, with my young people is to show them my own standpoint, but in no way to control their action. Unofficially and privately I shall always be ready to aid the Society.

Yours affectionately,

KARL PEARSON.

The avoidance of open conflict became however impossible when, directly after the issue of Elderton and Pearson's first memoir on alcoholism, Mr Montague Crackanthorpe, the Chairman of the Eugenics Education Society, wrote a long and antagonistic letter to *The Times*, which included such statements as the following:

"To those, however, who are familiar with the methods of eugenic...research the Report [i.e. (57)] causes no surprise at all. It simply confirms their belief that, serviceable as biometry is in its proper sphere, it has its limitations, and that a complex problem such as that of the relation of parental alcoholism to offspring is quite beyond its ken....

"First the biometrical method is based on the 'law of averages,' which again is based on the 'theory of probabilities,' which again is based on mathematical calculations of a highly abstract order. From this it follows that in this particular problem, biometric research supplies no practical guide to the individual...."

Galton himself felt it necessary to reply with a letter to *The Times* expressing complete dissent from the views of the Chairman of his Society, and he was almost moved to resign his honorary presidency. Putting aside the controversial aspect of the matter, the sentences from Crackanthorpe's letter that I have quoted illustrate a fact which it is always well for mathematical statisticians to bear in mind: the inevitable difficulty in putting across to the layman the sense in which the abstract theory of probability can be used as a guide to practical action. At bottom in this, as in other cases, the emotion underlying the attacks directed at Pearson and his biometric school by so wide a variety of critics was largely aroused by his claim that a mathematical technique, which they could not understand, was needed in the solution on scientific lines of the questions on which they considered themselves experts. Pearson did not seek for controversy; he knew how much time and energy it wasted. "Our policy is to work steadily away building up for the future. So long as the Mendelians do not attack us we shall leave them alone," he wrote to Mrs Weldon in 1907 on another occasion. Nevertheless his "capacity for roving into other people's preserves," coupled with his constant insistence on the need for statistically-trained minds, conveyed an implied criticism of the non-statisticians working in subjects they regarded as their own. While some sought the training that was needed, it was not surprising that many reacted in a different way. And once the dogs of war were loosed, Pearson gave blows as stoutly as they were given.

Two further memoirs were issued from the Eugenics Laboratory dealing with alcoholism ((59) 1910 and (60) 1912); they were concerned with a study of extreme

alcoholism in adults and gave particular attention to its relation to mental defect. The first paper, a joint work of Amy Barrington, Karl Pearson, and David Heron, dealt with data provided by Dr F. A. Gill, the Director of the Lancashire Inebriates Reformatory for women at Langho. The tentative conclusions reached by the authors were as follows: extreme alcoholism, like many forms of crime, was due to a want of will power and of self-control; it was therefore a consequence of the absence of mental balance, i.e. the consequence rather than the cause of mental defect as many persons had claimed. Mental defect, in many stocks at least, was an hereditary character. It followed that "segregation of the mentally defective child of both sexes was a first step in the effective treatment of both alcoholism and criminality." The authors also urged official recognition of the fact that "the prisons, the asylums and the inebriate reformatories form in combination a great national laboratory for the study of those degeneracies upon the limitation of which the welfare of society so largely depends."

The final memoir of the group (60), by David Heron, dealt with data from Inebriate Reformatories collected by Dr R. Welsh Braithwaite, the Inspector under the Inebriates Acts. From fuller data, Dr Heron reached conclusions regarding the relation of alcoholism to mental defect very similar to those of the preceding memoir. He also discussed in the light of these investigations the Government measures which had been or might be taken to deal with mental defect and inebriety.

The first paper issued from the Eugenics Laboratory on insanity had been written by Heron in 1907 (68). In this he considered the question of the inheritance of an insane diathesis, "a condition or state, which under suitable environment, the special mental or physical strain, ...may become one form or another of accepted insanity." The investigation, based partly on data obtained from an asylum at Perth, ran on similar lines to Pearson's investigation into the tubercular diathesis (53). A close correspondence was found between the intensity of inheritance of the insane and the tubercular diathesis, thus providing more evidence that tendencies to pathological defect were generally inherited in just the same manner as were physical characters. Further evidence on this point with regard to insanity was supplied by Goring in his memoir already referred to on the family history of criminals (63).

The most forceful contribution of the Eugenics Laboratory to the subject of mental deficiency belongs to a period a little later than that which I have been discussing, to the years 1913–1914, but some reference may appropriately be made to it here. The American Eugenics Record Office, after the collection and analysis of a considerable number of pedigrees and family records, had announced that there was little doubt that Mental Defect was a recessive Mendelian unit-character. On this assumption Dr C. B. Davenport, the Director of the Office, had written*: "At last it is possible to give definite advice to those about to marry, or who do not wish

* *Heredity and Eugenics*, University of Chicago Press (1912), p. 288.

to transmit their undesirable traits.... Weakness in any trait should marry strength in that trait; and strength may marry weakness." Although the danger from the social point of view of accepting this doctrine without prolonged and careful research should have been clear, the American work was regarded as of first-class importance among a wide circle of persons in this country. It was also seized on by the popular press, which spoke of the "entirely splendid work of the American Eugenics Record Office."

Pearson felt it to be essential to challenge the whole character of this work; in the first place it was necessary to show that even if mental defect was due to the absence of a Mendelian unit-character, or determiner, in the germ-plasm, Davenport's advice might lead in the long run to most undesirable consequences. A more critical study of family pedigrees showed, however, that it was not possible to fit the problem into the simple Mendelian scheme proposed; it was far more complex. Mental defect could not be regarded as a character which was either present or absent in an individual; as far as it could be measured by intelligence tests in children, there seemed to be a continuous grading with no sharp boundary whatsoever between the normal population and the populatior of children segregated as mentally defective. Finally, slipshod and uncritical work of this character by writers who had allowed theory to outrun knowledge was a serious offence against the infant science of Eugenics. The public, who in the long run had common sense, would put to the test such advice as "Let weakness in any trait marry strength in that trait, and strength marry weakness," would find that it failed and end by condemning wholly a science which proclaimed such absurdities.

The challenge was taken up in three pamphlets of *Questions of the Day and of the Fray*, by Heron (69) 1913, Pearson and Jaederholm (70) 1914 and Pearson (71) 1914. The artillery may perhaps have been unnecessarily heavy for its job, but those who have read the passages in Pearson's *Ethic of Freethought*, to which I have referred above*, will understand the deep sincerity which was associated with what he himself termed an "almost religious hatred" of error "propagated in high places." It will be well, I think, to set out here his own frank account of this aspect of the duties of a scientist, with which he prefaced his lecture on "Mendelism and the Problem of Mental Defect," delivered in February, 1914 (71):

"I am quite aware," he wrote, "that it is very bold for one who has had no direct experience of the mentally defective, either as a school medical officer, or as a teacher in a special school, to stand before you to-night and profess to give his opinion on the subject. But as I grow older I feel more and more the need not only for the *censores morum*, but for *censores scientiarum*, a species of watch-dogs of science, whose duty it shall be not only to insist upon honesty and logic in scientific procedure, but who shall warn the public against appearances of knowledge where we are as yet in a state of ignorance. In this age of self-advertisement, when an individual may become famous in twenty-four hours by aid of the illustrated daily press, there is quackery in science as

* Part I of this memoir, pp. 202—206.

there is quackery in medicine. And even where there is not quackery there is ignorance and dogma parading before the public as knowledge, and taking its toll from the community by a multiplicity of devices. In many ways the trained scientific mind can warn the public, even when it lacks acquaintance with specialized detail, and this is, above all, the case when the final problem turns on the interpretation of figures. To figures, in my experience, ultimate appeal is invariably made, and too often this appeal is in the inverse ratio of the power present of handling them. After all, the legitimate method in every branch of science is one and the same. The processes of observation and the material handled will differ, but the method of deducing a legitimate conclusion is common to all branches of investigation. It is summed up in the theory of logical inference, in the legitimate association of conceptions drawn from the facts observed. Unfortunately at the present time no theory of what we may term scientific logic is taught to students of science in our universities, and the result is only too patent in 50 per cent. and more of so-called scientific publications.

"I am fully aware that with so many tramps about the task of the watch-dog is by no means a pleasant one. He is thought to be quarrelsome for the fun of the fight, and writers rarely see both sides of a scientific controversy, or understand the almost religious hatred which arises in the true man of science when he sees error propagated in high places, and is told, forsooth, that he must not check this error by every means in his power for fear of hurting the feelings of Smith or Brown. There comes also a time when reasoning with error is absurd, when statements are so manifestly idle that they stand not by any force of observation behind them, but by the dead weight of authority. Then the only course open, the only thing which will kill obscurity is ridicule and sarcasm. Remember the years in which Erasmus, Reuchlin, and Agricola struggled by aid of reason alone to overthrow the scholasticism which choked all healthy growth in the mediaeval universities; then came the *Epistolae obscurorum virorum*—the everfamous letters reputed to be written by the obscure men, the scholastic theologians, one to another,—and within a couple of years the biting sarcasm of these letters of the younger humanists had freed the universities of Germany from their bondage. The renaissance had triumphed by the ridicule of obscurity, if the ground must first be cleared by the heavy artillery of scholarship and logic brought into the fight by the older humanists. To those who see the changes now taking place in the scientific world there must be a consciousness of a similar renaissance in progress. New scientific methods, new standards of logic and accuracy have fought their way to the front, and both in pure science and in medicine much of the work which may be done in the future on the old lines can only be looked upon as dogma or as quackery. The scientist and the scientific medical man have got to pass through the stage of saying *Ignoramus*, before they can safely assert that they begin to see clearly again." ((71) p. 3.)

Before concluding this account of the statistical investigations carried out during this period by Pearson or by those working under him, there are two further lines of research that must be mentioned, both of which were dealt with by the Biometric rather than the Eugenics Laboratory; they were concerned with the very different subjects of craniometry and astronomy.

The work of Quetelet and Galton in applying mathematical methods to anthropology led naturally to the application in craniometry of the biometrician's developing

statistical technique; only by the use of such methods was it possible to give precision to the characteristics of a race or to make a scientific determination of the reality of racial differences. As early as 1895 Pearson, with the help of Alice Lee and G. U. Yule, had made a series of measurements on the human skull and had calculated from these the constants of variability in man published in Pearson's *The Chances of Death* ((7) Vol. I, pp. 256—277). In the same year the first large collection of skulls and skeletons was sent to Pearson from Egypt by Flinders Petrie. This consisted of complete skeletons and skulls of over 400 members of the prehistoric Naqada race. A first investigation on this material by Ernest Warren was published in 1898*; a second report, mainly due to Cicely D. Fawcett with assistance from Alice Lee, but edited and arranged by Pearson, was published in 1902 in *Biometrika* (72). This last paper commences with a brief historical account of previous work and contains a statement of the objectives of biometric research in craniometry.

In the first place a precise definition of the characters measured was necessary. Since these characters were approximately normally distributed among the individuals of a race, any sample measured could be adequately described by the means and standard deviations and by the correlation coefficients between characters. The probable errors of these statistical constants would measure their reliability and make possible inter-racial comparisons. The correlation between two characters within a race provided little information, however, regarding the correlation between the averages of those characters in different races. While within a race the individual with a high value for a character x might tend to have a high value for a second character y and that with low x have low y, the race with high average for x might in general have a low average for y. Thus a full understanding of racial differences and of their bearing on the evolutionary history of man involved the patient accumulation of data. "A first step in this direction," the paper concluded, "should be to obtain the average values of some 40 or 50 characters in 50 to 100 races measured on some uniform plan."

From 1902 onwards a part of the energies of the Biometric Laboratory was directed to this ambitious task, as suitable series of skulls became available. Improved technique was evolved from time to time and many side issues were followed out, but this fundamental objective was kept in view. Among workers who, up to 1914, contributed in this field may be mentioned W. R. Macdonell, R. Crewdson Benington, in whose memory a Research Scholarship was founded in the Biometric Laboratory, Miss Dorothy Smith, Miss E. Y. Thomson and Miss K. Ryley. For the photography Pearson himself was largely responsible.

Much of anthropology is concerned with the study and comparison of groups. Yet it was not until Pearson's descriptive technique had been developed and tried out in many cases that it was possible to demonstrate the inadequacy of other

* "An Investigation on the Variability of the Human Skeleton: with especial reference to the Naqada Race discovered by Professor Flinders Petrie in his Explorations in Egypt." *Phil. Trans. Roy. Soc.* CLXXXIX, B (1898), pp. 135—227.

methods of approach then current, and to discover in just what sense the process of classification into groups could be carried out. Pearson was the first to insist on the necessity of obtaining large samples of skulls or bones; it was perhaps the evident impossibility of drawing sound inferences in this field from small samples that influenced his whole outlook on the general problem of small-sample theory. He also insisted on the collection of osteological material, partly because the measurements taken on the living are less accurate and partly because he felt that a profitable study of the relationships between existing groups depended of necessity on a study of their ancestral groups. In these ideas and their working out in practice lay his greatest contribution to physical anthropology.

Pearson's application of statistical methods to astronomy between 1906 and 1911 arose from his teaching of the subject in the Department of Applied Mathematics and also from his friendship with H. H. Turner of Oxford. He approached the matter with some diffidence, being "aware how badly the mere statistician may stumble in dealing with astronomical data," but he felt, I am sure rightly, that the method of correlation might provide useful exploratory tools in astronomical research. A joint paper of 1908 with Miss Winifred Gibson (73), following an earlier paper published by her in 1906, dealt with the inter-correlations of the stellar characters, colour, spectral class, magnitude, parallax and proper motion. A further paper of 1908 written by Pearson with assistance from Miss Julia Bell (74) discussed the correlation between light-range and light-maximum in various classes of double stars. This led to some discussion with H. C. Plummer—I purposely call it discussion and not controversy, for from neither side came any of the sting that accompanied the controversies in the field of eugenics referred to above—on the meaning of spurious correlation and the interpretation of numerical results*. There was an essential difference in approach that could hardly be bridged. I think we may see, too, a certain analogy with the differences that had characterised the outlooks of the biometrician and the Mendelian. The former believed that his tools, applied to mass data, could lead to the discovery of general relationships not otherwise possible to detect and he was convinced that such a discovery was a valuable preliminary to more detailed investigation regarding the individual unit. On the other hand to the geneticist, as to the astronomer, it seemed that the key to mass results could only be found by an increased knowledge of the structure of the individual model, whether this were a model of the germ cell or of the star.

(iii) *Statistical Theory*

In the '90's the biometric investigations into evolution and heredity had at times been held up because the development of statistical theory was unable to keep pace with the demands made on it by Weldon, ever full of the discovery of new and exciting problems. But Pearson's work through a decade which had opened with the theory of frequency curves and closed with that of χ^2, of contingency

* See *Monthly Notices of the Royal Astronomical Society*, LXIX (1909), pp. 128—151, 348—354, 573—585; LXX (1910), pp. 4—12, 228—229.

and of non-linear regression, had provided a technique which was competent to handle the main problems with which the biometric school was now concerned. It is not therefore perhaps surprising that in the years following the foundation of the Eugenics Laboratory Pearson made few major contributions to statistical theory. Two papers in the Drapers' Company *Biometric Series* may be mentioned.

The first, of 1906 (46), on "A Mathematical Theory of Random Migration" dealt with a problem of considerable biological importance. The immediate cause of the investigation appears to have been a problem regarding the infiltration of mosquitoes in cleared areas, put before Pearson by Major Ronald Ross. Clearly the theory had, however, many applications; the solution given was a first step which has led to further work by others*.

A second paper, (47) of 1907, was concerned with rapid methods of calculating correlation alternative to those based on the sums of squares and the product-sum. The expression for estimating the product-moment correlation coefficient from the correlation of ranks was obtained, as also an approximation to its probable error. It is interesting to note that we have here one of the first instances of the comparison of the probable (or standard) errors of two alternative sample estimates of a population frequency constant. Pearson was able to show that the product-moment coefficient had a smaller standard error than the coefficient obtained from ranks, except when the variables were uncorrelated in the population; in that case the standard errors were equal. He pointed out that for this reason, among others, the correlation coefficient should in general be calculated by the product-moment method. A situation might, however, occur from time to time in which a gain in speed would outweigh a loss in accuracy; the rank method would then be very useful.

Other theoretical papers will be found in *Biometrika*; several of these were concerned with methods of determining correlation from data classed, for one or both variables, in broad qualitative categories. For example one paper, (75) 1909, gives the method of "biserial-r"; while another, (76) 1910, describes methods of calculating the correlation ratio from data classified in broad groups, known sometimes as the methods of "biserial" and "triserial-η." The calculation of these coefficients from data of this character has more than once been criticised; in particular, doubt has been thrown on the meaning of the correlation coefficient estimated by the "tetrachoric" method from the four-fold or 2×2 table, a measure of correlation which certainly played an important part in some of the eugenic and biometric investigations to which reference has been made above.

To take an example from Goring's paper on the inheritance of the diathesis of phthisis and insanity ((63) p. 9), what meaning, it may be asked, and how much weight should be attached to the correlation coefficient, $r = 0{\cdot}44$, calculated by this method from the following table? The strict interpretation of 0·44 as a correlation coefficient involves the assumption of an underlying continuous variate, the tuber-

* See for example the paper by John Brownlee, "The Mathematical Theory of Random Migration and Epidemic Distribution," *Proc. Roy. Soc. Edin.* XXXI (1910), p. 262.

cular diathesis, following in the population sampled the normal or Gaussian distribution. It is an assumption which may or may not be accepted. But even if

Father

Children	Tubercular	Not Tubercular	Total
Tubercular	63	172	235
Not Tubercular	309	4862	5171
Total	372	5034	5406

it is rejected out of hand, we have still to remember that the tetrachoric r is a measure of relationship, lying between 0 and 1, which provides an ordered scaling of the intensity of association—in this case between tuberculosis in father and in child.

Where more categories were available, i.e. in the general case of an $h \times k$ classification, it was possible to apply very searching tests of consistency to the four-fold and other methods of estimating correlation, by using a variety of different classifications. The agreement found was satisfactory, particularly when, a few years later, Pearson's class-index correction was available ((77) 1913). Further, when these methods were applied to actual data for which the two variables were given on a quantitative scale, close agreement between the product-moment and more approximately estimated values of r was found. Finally, the fact that the correlation coefficients of inheritance in man found from data given in these two different forms showed close agreement, while it might have been a coincidence, undoubtedly gave much weight to Pearson's belief that the broad category methods were of great practical value. Nowhere, perhaps, were they tested so critically before acceptance as within his laboratories.

Of other papers during this period we may note one on inverse probability ((78) 1907) and another which showed how the χ^2 method could be used to determine the significance of the difference between two grouped samples ((79) 1911). There were a number of short notes published in *Miscellanea* of *Biometrika*, some of them dealing with points discussed in current lectures on the theory of statistics.

I have spent some time in a description of the research work of the Biometric and Eugenics Laboratories during this period between the deaths of Weldon and Galton; they were important years, when Pearson at the height of his power was hammering home the claim for the recognition of Eugenics as a branch of science worthy of academic study. But to make the picture I have attempted to draw more complete, it is necessary to give some account of more personal interests and relationships; to look inside the walls of University College with the help of some of those who knew him at that time.

"The Professor, as you may imagine," writes Miss E. M. Elderton, "was tremendously busy, and yet he always had time to sit down and discuss an individual problem. We

did not go to his room, but he came round at least once a day to see everyone. Even after a late afternoon lecture, he was glad to see visitors from outside, whether it was Dr Bulloch from hospital or Mr Gosset with his rucksack on the way to Euston and Holyhead....His enthusiasm inspired us all. I remember when he was working at albinism, on two occasions I saw albinos in the street and followed them till I discovered where they lived; then the Professor with his memory knew at once whether he had the pedigrees or not.

"I believe we gave the first public lectures in 1909; I remember his restlessness before a public lecture, he could not settle down to anything on the day and it used to comfort me a little to see this when I felt terribly nervous myself before one of my own lectures.... In 1909 we had an evening party made possible by our move into more extensive quarters in the College. There were several 20-minute talks on the work in progress. Nettleship gave an account of the albino dogs (there were dogs there in cages); Goring talked about his criminals and I think the Professor spoke about human piebalds. We had great fun over it all and tried to make guests take away forms to fill in—family schedules, forms about numbers in the family and cousin schedules. It was the first 'party' at which I helped, and I was struck by the fact that anything short of the best would not do for the Professor; labels for example must be very tidy and printed if possible. Later I used to think that we used too much time on such things; but I wonder if we did and whether it was not important to keep up the standard in small things."

Besides the permanent research staff, there was a steady flow of postgraduate workers who came for longer or shorter periods of training in the Biometric Laboratory. Such were J. F. Tocher, Major Greenwood, Raymond Pearl, "Student" (W. S. Gosset), J. Arthur Harris, W. F. Harvey, Charles Goring, H. E. Soper, E. C. Snow and Leon Isserlis, men who have since made their names in different fields of applied statistics. Others such as W. P. Elderton and W. F. Sheppard, whose friendship with Pearson had begun several years before, were in close touch with him during this period, though never actually working in the Laboratory. Some of these have already paid a tribute in print to the inspiration they received; I shall confine myself here to quoting two further impressions of these years, given me by "Student" and by W. F. Harvey of the Indian Medical Service.

It was in July 1905 that "Student" first consulted Pearson, riding over on a bicycle from Watlington to the farm at East Ilsley in Berkshire where the latter was spending his summer vacation, in touch with Weldon at Oxford.

"I had learnt what I knew about errors of observation from Airy," "Student" writes, "and was anxious to know what allowance was to be made for the fact that a 'modulus' derived from a few observations was itself subject to error. I also wanted to know what sort of error was attached to the clumsy method which I was using to show association (difference between $S(a+b)^2$ and $S(a-b)^2$); there were also other similar questions. Pearson was able in about half an hour to put me in the way of learning the practice of nearly all the methods then in use, ready for my work in London a year later.

"I am bound to say that I did not learn very much from his lectures; I never did from anyone's and my mathematics were inadequate for the task. On the other hand I

"K. P."

Extract from "*A Collegiad*":

'Tis sure the duty of the college bard
T' extol the drapers' houses by the yard,
I should say *in* the Yard or *on* the Green;
Where, too, a modest sundial may be seen
With which to tell the time on foggy days.
To guard it seemingly from Phoebus' rays
A bright-hued rabbit hutch is placed near by,
The work of Architecture's faculty.

From the *Union Magazine*, June 1907, by kind permission of the present Editors.

gained a lot from his 'rounds': I remember in particular his supplying the missing link in the probable error of the mean paper—a paper for which he disclaimed any responsibility. I also learned from him how to eat seed cake, for at 5 o'clock he would always come round with a cup of tea and either a slice of seed cake or a petit-beurre biscuit, and expect us to carry on till about half past six.

"Miss Elderton and L. F. Richardson made up the rest of the lecture class. Heron was there as demonstrator and Miss Barrington as computer. Goring came while I was there, but the visit with his fellow 'criminals' was later. Crewdson Benington came then too. At that time K.P. was also running the drawing office of the engineering school and was teaching astronomy; I have a caricature of him comparing his watch with a sundial that came out in the College paper*."

W. F. Harvey came to the Biometric Laboratory in 1908 when on long furlough from India. It was at a moment when Pearson was not only beginning to enlist the services of members of the medical profession in the collection of pedigrees, but to make some of them think seriously about the meaning of probable errors and significance. In this direction he was ably assisted by Greenwood, who was pushing forward with research in medical statistics. Harvey's impressions are therefore of special interest.

"The motives," he writes, "which determine a medical man to interest himself in work outside his own profession are, perhaps, not altogether clear to him himself. In my own case they may be set down, at least partially and somewhat crudely, to the mental disturbance caused to an ardent admirer and pupil of that outstanding figure in medicine, Sir Almroth Wright, by criticism of his work †. The appearance of a destructive analysis of the figures adduced in support of the successful use of typhoid vaccine as a prophylactic measure in the South African War came as a bombshell to the believer. The correspondence which ensued introduced terms of strange application to medical argument. One heard of 'correlation coefficients,' 'significance' of differences and 'selection.' A period of long furlough gave me the desired opportunity to probe further into the new instrument, which would bring a metaphorical foot rule to the measurement of causation and direct sequence in medical diagnosis, prognosis, prophylaxis and therapy.

"At the outset, and during my stay in the Biometric Laboratory, I now feel that preoccupation with mastery of details of calculation and technique obscured to some extent the full meaning and scope of the new science. The pleasure, however, of even that technical occupation was greatly increased by the opportunity I had of doing a double study, which might be described as a daily oscillation—it was a real physical one—between the spheres of influence of Sir Almroth Wright at the Inoculation Department of St Mary's Hospital, Paddington and Professor Karl Pearson at the Biometric Laboratory. That study began with attendance at 9 a.m. at University College and finished by the catching of the last train from Praed Street station back to my lodging. Both men who, as all know, have been doughty opponents took a keen

* This caricature, with an extract from a topical poem, is reproduced opposite. The "rabbit hutch" was not intended to screen the sundial but to cover a portable transit circle which was fixed onto the pedestal when required for student instruction.

† See *British Medical Journal* (1904), Pt. 2, pp. 1259, 1343, 1432, 1489, 1542, 1614, 1667, 1727 and 1775 for this controversy.

and kindly interest in my double dealing and to both of them I would now, at a much later date, tender the disciple's homage.

"These remarks are of the nature of autobiographical details. They have some significance, however, when one speaks of great men and may be the best tribute one can pay to either of them. It was inevitable that discussions should take place between pupil and masters on the subject-matter which was in dispute. I can remember well the exclamation of Professor Pearson to my suggestion that perhaps the testimony of 'experience' should not be eliminated from judgment upon a supposed cause and a supposed effect. 'Experience!' said he, 'I am always having experience thrown at my head.' Later reflection has robbed the remark of some of its original nakedness. This may be summed up by quoting the admission of another great debater of 'questions of the day and of the fray' with whom I also came in personal contact, Sir James Mackenzie, the physician. He was himself known to both of the protagonists in these lively discussions. 'Experience,' he came to admit, might be described as 'subconscious statistical arrangement of experimental data.' There we may leave the disputable subject."

The reader who cares to obtain a fuller picture of the way in which a medical man of balanced judgment could approach without bias, in those days of controversy, the relation of medicine and mathematical statistics, may turn back usefully to the paper which Harvey wrote as a result of his stay in the Biometric Laboratory*.

There is an aspect of Pearson's relations with some of his old pupils which cannot escape some comment. With many of them at some time or other during his long life he was in dispute. That intensity of purpose which carried him forward in an undeviating pursuit of what he believed was truth, brought him inevitably into conflict with the scientific views of several of his younger followers. On such occasions he felt that they were deserting a cause and he did not fully understand the effect that his strong personality had upon them. Both he and they valued independence of thought, but it was not easy for them to break free without overemphasising what was different at the expense of so much that was common; and once that conflict of opinion had appeared on some matter which Pearson regarded as fundamental, there was a danger that those strong emotions that moved beneath the surface and were beyond his complete control would lead to a coldness on his part, an "infelicity of expression" as he had described it to Galton†, whether in spoken or written word, which seemed to make personal a difference that should have remained in the field of scientific opinion.

Nevertheless I suspect that here, as elsewhere, it took two to make a quarrel, and those of his old students who were at issue with him by the way and yet ended as his friends will agree that there were faults on both sides. There were some indeed who had the skill to differ and to win their point; that was worth while! As one writes:

"I did not always agree with K.P. Generally of course I was wrong, but if I was right and convinced him, he was always pleased about it and I went on my way feeling

* "The Opsonic Index—A Medico-statistical Enquiry," by W. F. Harvey and A. McKendrick, *Biometrika*, VII (1909), pp. 64—95.

† See Part I of this memoir, p. 228.

'good all over.'...One problem, though suggested by him, was really more in my line than his; it was one of my lucky efforts when I discarded some methods of approach he had had in mind but which would not have worked and could not have given a satisfactory result. I remember being a bit anxious about it all. We had a talk when I produced arithmetical evidence; there was a pause and then he said, 'Yes, that must be right—of course you are right.' And I could have shouted for joy!"

Of friendships formed during these years, that with Charles Goring was among those which meant most to Pearson. It was a great testimony to the standing of the Biometric Laboratory that in 1909 H.M. Prison Commissioners decided to send C. B. Goring, who had been Deputy Medical Officer at Parkhurst, and two assistants, one of whom was H. E. Soper, to carry out in consultation with Pearson the statistical reduction of a long series of measurements on criminals. The observations concerned both physical and mental characters and were finally published in 1913 as a blue book, *The English Convict, A Statistical Study.*

Goring was a man of wide interests, a scholar and philosopher as well as a scientist, a born inquirer who mistrusted traditional face-values. He brought to the study of the criminal a power of careful observation and a warm humanity of a kind perhaps not often found together behind prison walls. There was something in his sympathy and understanding of his criminals, both in and out of prison, which reminded Pearson of Weldon, the naturalist, who had never been more happy than with his specimens in the field. The friendship, formed during those two and a half years when Goring was working regularly at University College, grew and developed afterwards until cut short all too soon by Goring's death from pneumonia in 1919, when battling with a prison epidemic of influenza.

I have said little of that friendship of a different kind between Pearson and Galton. Based in the first place on the admiration of a disciple for his master, it had grown more and more close in the years since Weldon's death. Testimony to this can be found in that long final chapter of the third volume of the *Life of Francis Galton.* The older man had above all provided the younger with two things, the outline of a new and powerful form of calculus and that great conception, which had so filled his later life, that "a true knowledge of natural inheritance might enable man to lift himself to a loftier level." In the last years his teaching days were over, but he could and did provide some of that wise counsel which Henry Bradshaw had supplied twenty and thirty years before. He also represented something else for Pearson; the one man who had a keen and enlightened interest in all forms of biometric work, to whom alone a Report of the work of his own Eugenics Laboratory was of capital importance.

In January 1911, in the same week in which Galton died, Pearson completed for the press Part I of the third edition of *The Grammar of Science* (12). It contained only the chapters on the physical branches of science, but there were included two new chapters. The first of these on "Contingency and Correlation—the Insufficiency of Causation" dealt with the author's outlook on that "category broader than causation, namely correlation of which causation is only the limit," to which Galton's

Natural Inheritance had twenty years before directed the young mathematician's attention. The second additional chapter on "Modern Physical Ideas" was largely contributed by E. Cunningham, at that time an Assistant Professor in the Mathematics Department at University College. Its relation to the main theme of the *Grammar* is indicated in the following quotation:

"The end of the nineteenth century, however, marks the advent of experimental knowledge requiring an entire revision of the hypotheses and theories as to the constitution of matter. In accordance with the main thesis of this work that our conceptual universe is merely the simplest logical construct into which we can gather all known perceived phenomena, the scientific mind must be prepared, as new facts of nature are brought to light, to examine whether or no they fit into the existing scheme. If they do, then the mental picture is thereby made a little more complete. If not, modification, enlargement, or even abandonment is necessary. The object of this chapter is to describe briefly the great revision that is necessitated by an unusual influx of new physical knowledge during the last twenty years." (p. 356.)

Part II of this edition, dealing with living forms, was never written; no doubt considerable addition to chapters IX, X and XI of the second edition was planned and it is clear from Pearson's Preface to Part I that he had hoped to complete this work during 1911. But new problems and responsibilities were to intervene. Whatever had been written on the biological sciences, however, in 1911, during that period of rapid development and change, must have borne a certain transitional character; nothing perhaps could have been published having the same permanent value as those nine chapters of the 1892 edition.

1911–1914

Francis Galton's death in his 89th year on January 17th, 1911, marked the end of a long and well-filled life. He had left his imprint on many branches of science, and towards the close the dominant idea of his life's work had crystallised into the conception of the linking of a new science and a new morality—for it was so that he regarded his Eugenics. It had for several years been his plan to leave the residue of his estate to the University of London for the endowment of a Professorship of Eugenics. The project had been discussed with Pearson from time to time since 1906; in particular in 1909 they had foreseen the difficulty that might occur of finding at once on Galton's death a suitable man for the post, who was young, full of energy and adequately trained in statistical method. For this reason, Galton had inserted in a codicil to his will a clause allowing the University to delay the appointment for a few years should they consider this advisable.

But Galton had another solution in mind; he saw in Pearson the ideal first holder of the Galton Chair and he realised how that appointment would release him at last "from the drudgery of teaching" mathematical and engineering students. He knew, however, that Pearson's first interest lay in the development of his training school in statistical method, the Biometric Laboratory and, fearing that Pearson would not regard the continued directorship of that Laboratory as consistent with

a loyal performance of the duties of a Professor of Eugenics, he added this final clause to his will:

"And I hereby declare it to be my wish but I do not impose it as an obligation that on the appointment of the first Professor the post shall be offered to Professor Karl Pearson and on such conditions as will give him liberty to continue his Biometric Laboratory now established at University College."

And so in the summer of 1911, after some doubts, after some negotiation with the University to ensure that there should be adequate funds not only for the salary of a professor but for the continuance of the existing organisation of the Eugenics Laboratory, Pearson relinquished the Goldsmid Chair of Applied Mathematics after twenty-seven years' tenure. He had enjoyed the work, he had learnt and he had taught, and in spite of a certain austerity of manner had won from his students all the popularity that attends a man who can hold and inspire large classes. But at the age of 54 he could have felt no serious regret at being freed to devote his whole energy to mathematical statistics, biometry and eugenics.

Many years before, Florence Nightingale had discussed with Jowett and Galton the founding of a professorship of what she had termed "applied statistics," which should be concerned with the application of statistical science to social problems*. The scheme had been dropped owing to lack of adequate funds, but the ideas discussed at that time had no doubt borne fruit in Galton's mind, and now Pearson felt that the term was an appropriate one to adopt. He was Professor of Eugenics, but the organisation of which he was in charge was concerned with a wider field; thus the Biometric Laboratory, supported by funds from the Drapers' Company, and Galton's original Eugenics Laboratory became incorporated in a new Department of Applied Statistics.

It was a research institute in the making. "There is undoubtedly work enough for two professors," Pearson wrote a few years later in a Report to the Drapers' Company, "...one to carry on the pure statistical work and biometry and the other the Galton Eugenics Laboratory. That indeed should be the goal aimed at, but it is an ideal of a distant future." The Department had yet no suitable accommodation, no adequate endowment for staff or publications, nor the funds that were needed to secure the effective attainment of the objectives which Galton had outlined in his will:

(i) Collect materials bearing on Eugenics.

(ii) Discuss such materials and draw conclusions.

(iii) Form a Central Office to provide information, under appropriate restrictions, to private individuals and to public authorities concerning the laws of inheritance in man and to urge the conclusions as to social conduct which follow from such laws.

(iv) Extend the knowledge of Eugenics by all or any of the following means, namely: (*a*) Professorial instruction, (*b*) Occasional publications, (*c*) Occasional public lectures, (*d*) Experimental or observational work which may throw light on eugenic problems.

* For an account of this episode, *The Life, Letters and Labours of Francis Galton*, (18) II, pp. 414–424, may be consulted.

In October 1911 the University issued an appeal for funds for the building and equipment of a Francis Galton Laboratory. Part of this appeal was almost at once met by the generous offer of an anonymous donor, later known to be Sir Herbert H. Bartlett, to provide a building for the combined laboratories on the Gower Street frontage of University College in continuation of the School of Architecture already under construction. The site was not altogether ideal, partly from the point of view of noise and partly because it left no possibility of later expansion, nor room for an adequate animal house for experimental breeding. But the offer was accepted; it marked a big step in the building up of that institute of which Pearson dreamed. Here is a letter written to Mrs Weldon at the end of 1912:

7, Well Road,
Hampstead, N.W.
Dec. 25, 1912.

My dear Friend,

Bed on Xmas day enables me to send a greeting to one or two old friends. Wife and bairns are at their Aunts at Highgate and I am quite peacefully convalescing from an attack of lumbago....I have read quite a lot of novels, etc. Also a couple of Greek tragedies in translation and George Meredith's letters. I am sending you the last of the index to the mice*. We shall now have to print the key and the notes on the individual mice.

I have on the whole good news as to the Laboratory. We have £3,800 from the public subscription. The anonymous donor has now offered to take the cost of building on the street front, less £3,000 to be provided by the College. This means that the donor will give about £12,000 for Eugenics and Biometry buildings and we shall spend about our whole £3,800 on equipment. We shall have a large three-storied building—not indeed in an ideal situation, i.e. on the street frontage—which is noisy and not the best for breeding work of any kind. But to have a building at all will be a great achievement. Once get this and then we can go forward to the other things I dream of!

I want to see a doubled staff with a zoologist and a medical officer and a biometric farm, such as we used to plan in the good old days! How he and I could have worked it out together, if the fates had been on our side! And now one is growing too old!...

Yours always sincerely,
KARL PEARSON.

The building itself was completed in 1914 and should have been occupied, fully equipped, by October 1915. But these plans were upset by the war.

I do not propose to discuss in any detail the research work carried out in the new Department of Applied Statistics during the three years 1911–1914. Several lines of inquiry had already been initiated in earlier years and have been described in the preceding section. To carry out the objectives set out in Galton's will, fresh supplies of the raw material for eugenic research were needed. For this purpose contact was made with the Medical Officers of Health in various large towns; data were obtained, for example, from Sheffield, Bradford, Liverpool, Glasgow and

* A reference to the completion of the reduction of Weldon's mice data.

Rochdale and in certain cases, in return, a public lecture dealing with the material supplied was given in the town by Pearson or a member of his staff. It was many years before some of this material was finally reduced and published. The following investigations undertaken, if not completed, at this period may be mentioned: (1) A co-operative study, "On the Correlation of Fertility with Social Value" (80). (2) Miss E. M. Elderton's "Report on the English Birthrate" (81). (3) An investigation based on an extensive physical, mental and medical examination of the children at the Jews' Free School in Aldgate, London, made in combination with reports from "field workers" on home conditions. The conclusions drawn from this investigation were not published until some years after the war (82). (4) An inquiry "On the Relative Value of the Factors which influence Infant Welfare" by Miss Elderton, based on data supplied by the Medical Officers of Health at Rochdale, Bradford, Blackburn, Preston and Salford. This also was not completed until more than ten years after the data were collected (83).

Besides the occasional lectures given outside London to which I have referred, a course of public evening lectures on the work of the laboratories was given at University College every winter. The following list is taken from the syllabus of one of these courses:

THE FRANCIS GALTON LABORATORY OF NATIONAL EUGENICS.

Session 1913–1914.

A course of six public lectures given on Tuesdays at 8.30 *p.m., commencing on Tuesday, February* 10*th.*

Lecture I. On the graduated character of mental defect and on the need for standardising judgments as to the grade of Feeble-mindedness which shall involve segregation. By KARL PEARSON, F.R.S., Galton Professor.

Lecture II. On some further points in connection with the fall in the Birth-rate. By ETHEL M. ELDERTON, Galton Research Fellow.

Lecture III. Infant mortality in a manufacturing town. By ALICE LEE, D.Sc., Research Lecturer at Bedford College, formerly Assistant in the Biometric Laboratory, University College.

Lecture IV. An examination of some recent studies of the inheritance factor in Insanity. By DAVID HERON, D.Sc., Assistant Director of the Galton Laboratory.

Lecture V. On the handicapping of the Firstborn. By KARL PEARSON, F.R.S., Galton Professor.

Lecture VI. On some recent misinterpretations of the problem of Nature and Nurture. By KARL PEARSON, F.R.S., Galton Professor.

Pearson's three lectures, I, V and VI, were afterwards published separately (71), (84) and (85)*.

* The dates of the lectures given in these publications do not correspond with those on the copy of the syllabus from which I have quoted; possibly the order of the lectures in the course was changed after the syllabus was issued.

Lecture V on the handicapping of the Firstborn dealt with a problem to which Pearson had referred in several earlier publications. Was it a fact that the earlier-born members of a family were to some extent inferior physically or mentally? If so, the limitation in the size of families which was spreading throughout the civilised world must by itself tend to increase general degeneracy. He had come to the conclusion that there was definite evidence of a slight "handicapping" of this character. The evidence upon which he based this conclusion had been criticised, mainly on the ground that a comparison of the orders of birth among individuals selected by virtue of their being marked in some way, e.g. by having some defect, with the orders of birth in the sibships to which they belonged, was inexact. The critics advocated a different method of comparison. Pearson was confident that he was right and his critics were equally confident that he was wrong. There we may leave the matter, since an intelligible discussion of the rival arguments would take us into detail beyond the scope of this memoir.

Lecture VI on Nature and Nurture was an emphatic restatement of the conclusion which Galton had reached long before his Laboratory had been founded, that heredity had a much greater power to determine the character of man than had environment. The lecture was illustrated by data on child welfare supplied by the Medical Officers of Health already referred to.

These lecture courses were attended by audiences both keen and critical. Social reform was in the air; a Liberal Government firmly established in Parliament seemed inclined to take an active part in social legislation; philanthropists, scientists and philosophers aired their views freely on the platform or in the press. The conception, or a misconception, of Eugenics had inevitably caught the popular attention; and while much rubbish was written and spoken, there was evidence, to be found in the pages of the weekly or quarterly reviews or sometimes in those of *The Times* and *The Morning Post*, of a thoughtful public which appreciated the value of careful and unbiased scientific inquiry on problems that must be of far-reaching social importance.

"The province of Eugenics," wrote a leader writer in *The Times* on October 7th, 1911, when supporting the appeal for funds for a Francis Galton Laboratory, "is not to yield to first impressions, but to get down to the bedrock of facts, and to arrive at correct appreciations of their value and meaning. The graver the social conditions surrounding us appear in their first aspect, the more important does it become that they should be thoroughly investigated, and that legislators and reformers should submit themselves to the guidance of knowledge in attempts to deal with them. The state of morals and of intelligence disclosed by the recent strikes, the state of health of the rising industrial population as disclosed by the medical inspections of schools are alike in showing the need for the study and the application of Eugenics, and in affording support to the appeal which we bring before our readers. It is becoming plain that the scientific investigation of the facts concerned can only be neglected by politicians who are in a hurry to introduce 'popular' reforms, and that, even with them, the neglect is more than likely to bring a Nemesis in its train."

The lectures were illustrated by many striking wall diagrams; these were at first largely planned by Pearson himself, who brought the experience acquired in the engineering drawing office to the handling of stencils for printed lettering and the arrangement of huge pedigrees which sometimes covered the whole wall of the lecture theatre. Later H. E. Soper and Miss Gertrude Jones brought fresh talent into this important field of clear diagrammatic presentation.

Of Pearson's contributions to statistical theory between 1911 and 1914 the following may be mentioned:

(1) A paper of 1912 in the *Drapers' Company Biometric Series* (48), "On a Novel Method of Regarding the Association of two Variates classed solely in Alternative Categories." This contained an ingenious suggestion for calculating a measure of correlation from such data by transferring to a correlation scale the probability measure obtained from applying a χ^2 test for independence to the 2×2 table. The conception was novel; it may have originated from a search to avoid the assumption of an underlying Gaussian distribution, inherent in the tetrachoric method of calculating correlation. Abacs constructed by H. E. Soper made the computational procedure quite short, but the idea involved in the theory was not altogether simple and I do not think the method has ever been widely used.

(2) Three papers of 1913 published in *Biometrika*, Vol. IX: (*a*) On the probable errors of frequency constants (86); (*b*) On the probable error of the tetrachoric coefficient of correlation (87); (*c*) On the correction to be applied to measures of correlation calculated from data classed in broad categories, a paper to which I have referred on p. 181 above (77).

If Pearson's output of purely statistical work during these years was reduced, there was good reason. The task of writing a biography of Galton had been entrusted to him by Galton's relatives soon after the death of the latter. It could in no case have been an easy task, for to describe adequately the work, the travels, the friendships which had filled a long life of nearly 89 years must have needed much patient delving and reading. But to Pearson the undertaking was one of special significance and the standard which he set himself led him to plan out a programme which, partly it is true owing to circumstances he could not have foreseen, was not to be completed for nearly twenty years. "My object," he wrote in the Preface to Volume I, "...is to issue a volume to some extent worthy of the name of the man it bears—which may be studied hereafter by those who wish to understand him, his origin and aims...." It seemed to him in the first place peculiarly fitting to place on record some account of the ancestry of the author of *Hereditary Genius*, *English Men of Science* and *Inquiries into the Human Faculty*, all books "essentially devoted to the thesis that mental characters are inherited in the same manner and at the same rate as the physical characters." In following this course he was brought inevitably to link up Galton's ancestry with that of his first cousin, Charles Darwin,

and to attempt to trace among their forbears the many characteristics, sometimes common and sometimes different, which had marked these two great Victorian scientists. A considerable portion of the first volume of the *Life*, published in 1914, was occupied with this part of the task; it was illustrated by pedigrees and many family portraits.

"To follow step by step backwards the pedigree of one man like Francis Galton," Pearson wrote ((18) I, p. 11), "till we can go no further, but find all our lines fail us, is perhaps the most instructive lesson in history that is possible. The biographer has learnt more history, social and political, in the present inquiry than he had ever done before. One sees not only our own times linked up with great names in the past, but one feels that yeoman, squire, noble and king form a more homogeneous whole than we have hitherto appreciated with our narrow class distinctions; and we realise that the stocks which led to famous men of old may exhibit them to-day in methods more in keeping with our social ends."

It was therefore with some triumph, as artists happy in their creation, that Pearson with his collaborators, Miss Barrington and Miss Jones, must have regarded their great completed pedigree which ran back from Darwin and Galton to William the Norman, to Alfred the Saxon, to Charlemagne the Frank, to the Kings of Scotland and the Emperors of Byzantium.

If Pearson could have followed his original scheme, the biography would have been associated with an issue of Galton's collected works, which would have made the description of his many researches far easier. When, after the war, the great rise in printing costs made the plan impossible, Pearson decided that his own two later volumes of the *Life* must include a résumé of memoirs, book and articles, which had been scattered widely throughout the pages of the publications of many learned societies and scientific journals and had sometimes since become inaccessible. Only in this way, he believed, would later readers be able to appreciate what Galton had done and to pick up many suggestive lines of thought where he had dropped them.

There was another side also to this long three-volumed *Life*; Pearson enjoyed the writing of it and the contact into which it brought him with the many sides of Galton's mind. As he tells us in the Preface to the last volume written in 1930 at the close of this great labour of love:

"It may be said that a shorter and less elaborate work would have supplied all that was needful. I do not think so, and there are two aspects of the matter to which I should like to refer....I have written my account because I loved my friend and had sufficient knowledge to understand his aims and the meaning of his life for the science of the future. I have had to give up much of my time during the past twenty years to labour which lay outside my proper field, and that very fact induced me from the start to say, that if I spend my heritage in writing a biography it shall be done to satisfy myself and without regard to traditional standards, to the needs of publishers or to the tastes of the reading public. I will paint my portrait of a size and colouring to please

UNIV. OF WICH.

"Nevertheless, my head is so full of chalk-downs and clouds, and things, I can't write biometry to-night. Always, when I have been with the country, the feeling breaks out that the other folk have the best of it. The other way you live with the country and become part of it; and you dredge, or fish, or shoot something wonderful, and you describe it, and everyone sees that it is wonderful, and you all enjoy the wonder. And there is no solution, and if there were, it would not be worth the shadow of a shower flying across the country.

And this is all wicked nonsense, and I am going to bed."

W. F. R. WELDON
Letter, April, 1903

DESSERT IN COMMON ROOM

TOASTS

1. IN PIOUS MEMORY OF SIR FRANCIS GALTON
2. IN REMEMBRANCE OF ALL BENEFACTORS
3. IN MEMORY OF THE BIOMETRIC DEAD
4. THE GUESTS
5. THE POSTGRADUATE WORKERS
6. THE PROSPERITY OF THE LABORATORIES

COFFEE IN THE MUSEUM

W. F. R. WELDON, 1860–1906
From the bust by H. R. HOPE PINKER

From the toast-card of the Seventh Anniversary Dinner, Galton and Biometric Laboratories. *February* 22, 1926.

myself, and disregard at each stage circulation, sale or profit. Biography is thankless work, but at least one can get delight in writing it, if one writes exactly as one chooses and without regard to the outside world! In the process one will learn to know—as intimately as any human being can know another—a personality not one's own; that is the joy of spending years over a biography where there is a wealth of material touching the mental output, the character and even the physical appearance of the subject."

In the three volumes of the *Life* there were reproduced from photographs, sketches and paintings nearly forty portraits of Galton. To Pearson one of the highest functions of the painter or sculptor was to catch and hold for later generations some part of the personality of the great men and women of the day. He felt that there was something which the artist with his brush or chisel could achieve that lay beyond the power of the writer with his pen. It is in this idea that I think we may trace the origin of Pearson's friendship and admiration for H. R. Hope-Pinker, the sculptor. The full-size statue of Darwin in the Museum at Oxford, a photograph of which formed the frontispiece to Volume I of *Biometrika*, and which still figures on the standard buckram binding-cases of this journal, had been carried out by Hope-Pinker. After Weldon's death, the work of modelling and casting a bust of him in bronze, also for Oxford, was entrusted to this same artist, who had perforce to work only from photographs.

"Don't mind if it is not a great portrait—it hardly can be—," Pearson wrote to Mrs Weldon, "but if he gives a work of art, which portrays a man of intellectual strength and keen mind, then be happy. It will associate your husband's memory with an ideal for future generations, who won't care much what any of us were really like in the flesh. The portrait will not live, but the ideal man of science embodied in a real work of art will."

There is a letter written to his son at school in 1912 which gives a picture of the lighter side of some of Pearson's many activities, the Galton *Life*, sculpture, dogs, a Royal Society soirée and the investigation into the temperatures of school children:

7, Well Road,
Hampstead, N.W.
May 25, 1912.

My dear old Boy,

I don't often bother you with a letter, and I have no paternal advice to give, but I thought I might write one of Mother's three weekly letters. Old Samuel Galton found one of his sons in trouble over something, and said to him: "Tell your friend Sammy all about it, and he'll say never a word of it to your Father"—and I think the old Quaker's division of the Father into business and gossip was rather good. Well this is gossip! Of course dogs come first; they have had no Saturday walks with me because I have been going to Hope-Pinker, who asked very nicely to have a study of my head for some bust he is making of Roger Bacon!! Now Matthew Paris tells us that a "quidem Rogerus Baconus, clericus de curia," was jocund and merry and fat—and the said sculptor has got a veritable Cassius both in his model and his actual study! But it

is interesting to see a big man at work, and envy his powers; there was such a shade of difference only when after working two hours on the first day he put the callipers to all parts of my head and tested on the clay itself. His appreciation of lengths *in space* must exceed that of a good draftsman on a board; we usually get to a millimetre or two in the latter before the scale is applied. E.g. is I should say about 3·2 cm. Well, Hope-Pinker does that sort of thing *in space*.

Mr Usher kindly offered me Donald dhu, but I have nowhere to put or keep him, otherwise I should have accepted him straight off. He would run so nicely with a white dog. Perhaps some day we shall have place for him.

About Mr Croft, please give him a message from me*: (i) I suppose he saw me upstairs, but I did not see him, i.e. I may have seen him but did not recognise him. I was only upstairs in a hurry as I could not leave my dogs. (ii) Why did he not come down and talk to me and the dogs? We were all pining to see people and did not get very many as we were in an out of the way corner, behind the cloakroom. Don't forget to tell him this, and I should immensely have liked a talk with him, of course not-at-all about you!

Have the temperatures been rising in Winchester lately? I wonder if I shall get a bad name or whether you will all consent to the torture of being taken in view of the aim: i.e. to find out whether the children of the poor are really in such a bad condition as our temperature observations in their schools seem to show. I must not now write more, or I shall get no work done this morning.

Ever your affectionate Father,

K. P.

The Weldon bust had been one item of a memorial scheme initiated shortly after Weldon's death; the greater part of the funds collected were handed over to the University of Oxford to found a Weldon Memorial Prize in Biometry which is now awarded every three years. The first award was made in 1912 to Pearson, but he would not accept the honour and the reasons which he gave express characteristically what he felt on the subject of prizes and medals:

"I do fully appreciate ..the desire of the Electors, but you know that I knew Weldon very closely and can still feel what he would think and say. The Darwin medal came to me when I was relatively young and it encouraged me as a young man and made me feel that medals and prizes might be helpful to young men, directing their energies and telling them that they were appreciated. The R. S. usually gives its medals to old men, whose reputations are already made, it gives them momentary pleasure and saves the R. S. much trouble in selection, but from the standpoint of science the medals are idle. Now what I have written down is what W. F. R. W. would have said, and how he acted when he proposed my name for the Darwin Medal. The Weldon Medal must go to encourage young men if it is to be fruitful to science. I am old now, and medals or no, I shall go on with the little work I still can in Biometry to the end, but a young man or two may be preserved for work in that direction...."

* This is a reference to a Royal Society soirée, at which the Galton Laboratory had an exhibit of albino and coloured dogs, and which was attended by W. B. Croft, the Winchester Physics master.

The year 1914 saw the completion of a long-planned project in the issue of *Tables for Statisticians and Biometricians* (88). The tables had been calculated over many years and largely published, as opportunity offered, in *Biometrika*; as they were printed, they were also moulded, in order that stereos might be taken for reproduction.

"From the beginning of this work in 1901," Pearson wrote in the Preface, "when the first of these Tables was published and moulded, I have had one end in view, the publication, as funds would permit, of as full a series of Tables as possible. It is needless to say that no anticipation of profit was ever made, the contributors worked for the sake of science, and the aim was to provide what was possible at the lowest rate we could. The issue may appear to many as even now costly; let me assure those inclined to cavil, that to pay its way with our existing public double or treble the present price would not have availed; we are able to publish because of the direct aid provided by initial publication in *Biometrika* and by direct assistance from the Drapers' Company Grant."

It was in this spirit that the whole of the publications of the Biometric and Eugenics Laboratories were issued during Pearson's long directorship. There were never profits and the cost, to the reader, of tables, pamphlets or memoirs, was worked out on a basis that would do little more than ultimately repay the cost of printing. The Tables of 1914 were regarded as a first contribution to a more complete series; among other things Tables of the Incomplete Gamma and Beta Functions were already projected. The Preface gave an opportunity of thanking the many loyal friends and colleagues who had ground their Brunsvigas to the appointed end—W. F. Sheppard, W. P. Elderton, Alice Lee, P. F. Everitt, Julia Bell, Winifred Gibson, A. Rhind, H. E. Soper and others. Warm thanks, too, were offered to the staff of the Cambridge University Press. "To those who have had experience of numerical tables prepared elsewhere," the Editor could write, "the excellence of the Cambridge first proof of columns of figures is a joy, which deserves the fullest acknowledgement."

And so in the early summer of 1914 the auspices for the future of biometry and eugenics were good. However much he might himself doubt his power of continuing much longer at work, the Head of the Department of Applied Statistics, even at 57, seemed to his friends as young and full of vigour as ever. A spacious new building was nearing completion across the College quadrangle; funds for its equipment were in the bank, and plans for its Anthropometric Laboratory and Museum were being eagerly discussed. The Laboratory publications, whether in lighter or heavier vein, were purchased in increasing numbers, the sales bringing in some £250 a year; *Biometrika* was at last on a sound footing and paying its way. Courses of public lectures were well attended; though sometimes hidden behind a screen of controversy and of journalistic popularisation of the concept of eugenics, a growing body of opinion was learning to appreciate the value of statistical method. And then on those halcyon days, which to many of us now form only a dim background to our school-days, burst the thundercloud of European war, destroyer of so many hopes.

1914–1920

With Volume I of the *Life of Galton* and the *Tables for Statisticians* off his hands and through the press, Pearson had decided to break his usual summer vacation habits. Perhaps it was the discussion of Galton's *Wanderjahre* in the *Life* that caused him to revisit the Black Forest in search of the landmarks and faces that he had known so well in the '80's. Whatever the cause, early July found him established with his wife at his old haunt, the *Gasthaus zum Ochsen* at Saig, near Lenzkirch.

"Into this small society," he wrote*, "we settled down and thought neither of war nor of racial feeling. We shared our newspapers, collected scarce flowers with the German boys, or discussed where bilberries or wild strawberries were to be found. We reported at meal times how many wild deer we had startled, or planned mild expeditions to neighbouring villages to drink our coffee or test their cake. We discussed food, perhaps a trifle too insistently, but perhaps not so continuously as we should have done thirty years ago. The *Bürgermeister* was a well-to-do peasant, and he, and an old friend the *Bierwirth*—with whose father I had years ago a common interest in the pursuit of trout—would spend time in watching the upward progress of their new *Rathhaus*.... There was confidence and friendship on all hands; the local banker cashed readily my circular notes, and we waited the days when our family from college and school would join us.

"Then suddenly fell upon us the blow of Tuesday, July 28th—the declaration of war by Austria against Servia—which, to my mind, left no loophole for diplomacy to save the situation...."

After describing the resentment of the local peasantry and many of the visitors when the *Bürgermeister* announced that they were in *Kriegszustand*, he went on to tell of a broken journey home by train along the Rhine; of an increasing popular war-fever in the industrial Rhineland; of the insistent question in the train, "*Was macht England?*" of the crowd which rushed down the platform at Crefeld shouting "*Ein Russe, ein Russe*"; of crossing the frontier into Holland with luggage on a barrow; and of final arrival in London, tired but without loss of property, on August 4th, the day that Great Britain declared war. It was a strange chance that had sent him to Germany after twenty-five years, in this year of all years!

The new situation was promptly faced by the members of the staff of the two Laboratories; the right course, they agreed, was to give up their immediate programme of research and to place their calculating powers at the disposal of some Government Department. Within a few days, Pearson had arranged with the Board of Trade to set in train and carry on a piece of work for the Labour Exchanges and Unemployment Insurance Department. The scheme was to provide and keep up to date charts showing the state of unemployment among insured and uninsured workers, male and female, for each town in Great Britain of over 20,000 inhabitants.

* The quotation is taken from an article "Germany in the Eye of an Onlooker," contributed to *The New Statesman* of 22 August 1914.

These charts were required by the Department and various Relief Committees. At the initial stage something like 50,000 percentages had to be calculated from data supplied by the Labour Exchanges and 2000 curves drawn on solid paper and then transferred to tracing paper for blue-printing. The staff, to whom was added a motley crew of some ten or twelve volunteers—engineering demonstrators, teachers, undergraduates—set about the big task with a will, and by mid-September the work was settling down into the routine form in which it continued until the following June.

Such was the first piece of war work; there were many others undertaken in the next four years, culminating in the big programme of gunnery computation. I shall let the account of this work stand in Pearson's own words, in the Report he made in February 1918 to the Worshipful Company of Drapers on the "War Work of the Biometric Laboratory." This Report is given below in Appendix III. It was written, as were other similar Reports, not only as a statement of work done, but to persuade the Drapers' Company to renew their grant*.

The programme of lectures in the Department of Applied Statistics for the Session 1914–15 had included a new departure in the shape of an introductory evening course to be given in conjunction with the Department of Zoology. In the first term Dr C. H. O'Donoghue of the latter Department was to discuss "The Biological Basis of Heredity"; in the second and third terms Pearson was to deal with "The Statistical Basis of Eugenic Theory" and "The Facts and Theories of Heredity." The syllabus of Pearson's first course, with its "Games of Chance, Tossing, Lotteries....The beginnings of Statistics; Graunt, Petty....The Universe regarded as a system of correlated organic and inorganic factors; the category of correlation as replacing the conception of causation..." is reminiscent of the syllabuses of the Gresham Lectures, given twenty years before. But the scheme, alas, was still-born.

"About eleven persons besides the Laboratory staff attended O'Donoghue's first lecture, 'a most excellent discourse' as Pepys would say, and of these two or three were merely first nighters and the remainder chiefly ladies diligently knitting for the soldiers. There was such a contrast to our usual throng at public lectures and so little public desire for eugenic instruction at this time, that the lectures were postponed *sine die*."

Now, long after, with a knowledge of Pearson's fifteen years of vigorous post-war directorship, it is perhaps difficult for us to realise the most personal aspect of the strain and depression which for him accompanied those war years. Within him there was a continual struggle between loyalty to his country and its cause, which he had at heart, and loyalty to the objectives of his own life's work. A younger man could have thrown biometry and eugenics from his mind for the period of the war, confident in his own power to pick up the threads where they had been dropped. But Pearson, weary after a ten-hour day on bomb or shell trajectories, could feel little confidence in the future. Would he be there to set going that

* His dislike for this recurrent task of report writing is expressed in a comment on one of these occasions, "horribly self-laudatory, how I hate the doing of this sort of thing!"

research institute of his dreams in the building now occupied by convalescent soldiers; or must all the threads that led back to Weldon and to Galton be broken? With the old trained staff scattered and attracted into other fields; with *Biometrika* perhaps closed down for lack of funds; with a new Galton Professor in charge, might not the war gap cut finally across the line of tradition and bring to nothing all those years of effort?

In the first year of war the strain was increased by uncertainty as to the future of the new building; it was first stated that the War Office wanted to make use of it as an extension of the space allotted to the wounded in University College Hospital; then the scheme was dropped and plans for its equipment as the Galton Laboratory were brought up again, only to be dropped in turn when the War Office returned to its original project. In the planning and counter-planning which resulted, the members of the College Committee and of the Galton Committee, with the exception of Galton's nephew and executor, Edward Wheler-Galton, each no doubt absorbed in his own troubles, did not understand the struggle that was tearing at Pearson's heart.

With his own staff, too, it was at times difficult. In keeping them together as a trained unit he was following a sound practical course, as well as thinking of the future. But he could not provide the salaries that were being paid elsewhere for other Government work, and in the restlessness of those days the routine of computing and drawing charts of unemployment or of imports was not felt to be "real war work." The calculation of shell trajectories was more nearly the genuine thing, but that was not undertaken until later on. And so, while he gave his blessing to each leaving member of his staff, the loss left him with some feeling of bitterness, for old colleagues seemed to be deserting the cause which was such an essential part of his life.

The following extract from a letter to a friend was written on January 1st, 1916, after a relatively free period between two spells of war work:

"All last session we did nothing but Government work, and this term and the vacation I have been entirely occupied with individual people's work. I had to get endless papers ready; the younger generation have to push their way forward, and publication of their work is immediate and essential to them. I have nobody now, but myself, who can even whip a paper into decent form for press. In the part of *Biometrika* just out are two memoirs which I wrote up almost entirely and for which I did all the photographs. I don't complain of this and I don't want other people to know, but they mean all the work I have done in four months and I want four months for the mouse paper* and six months for the second volume of Galton—and meanwhile any work of my own gets pushed to the Greek Kalends! Meanwhile too I have volunteered to put the whole Laboratory staff again at the disposal of a Government Department and do not know how soon we may be called off all work again.

"People are writing to me about Vol. II of the Galton, as if I were a criminal for not having issued it! But they do not stay to ask what it cost in time or money taken from

* This refers to the completion of Weldon's mice work.

other things. One man wrote about 'its outrageous price,' when it would not have paid for itself had all copies been sold, and when practically the book was a failure owing to the outbreak of war. Even *Biometrika*, which seemed safely in port after stormy years, has had a frightfully bad time during this war year, but I am told that other scientific journals are in worse case*."

Among the relaxations that Pearson found in these dark years, there were two that may be mentioned; the quiet atmosphere of a cottage in the country and the stimulus which he could get by turning from guns and bombs to research into the theory of statistics. Early in 1916 he decided to look out for a week-end cottage which would provide his family and himself with a breath of fresh air in the middle of strenuous occupations. An advertisement in *The Observer* sent him down one snowy Sunday to climb Leith Hill in Surrey and to find at Coldharbour the little house which was to be his, first as tenant and then as owner, for the next twenty years.

The position of the Old Schoolhouse, as it was called, under the crown of the hill with the commons and pine woods behind and in front the wide view over the weald of Sussex to the South Downs, grew on him more and more with the years. In those first days they were cutting the larch woods for pit-props; the smell of the timber reminded him of his loved Black Forest; there was a camp of German prisoners engaged on part of the work and a chat with these men helped a little to throw into perspective that bitter struggle going on across the Channel. The country, too, was made familiar from the word-pictures of George Meredith; Box Hill, the yew trees, the wild cherry, the seed of willow herb....Though born in London, Pearson had in him a strain of the essential countryman, inherited or acquired from his father; a love of the pure air, of watching the habits of birds, the growth of trees and flowers, the changes in the clouds and sky. Ten years before he would have linked this bent with some line of biometric research. Now he felt too tired and pressed; but it gave him immense joy to scent the pine woods as he climbed the hill from the station on a Friday or Saturday night after a full week's work in Gower Street; or, on a short Easter holiday, to prepare the ground in the small garden plot and sow his lines of peas and beans and potatoes.

The death of W. R. Macdonell in 1916 deprived Pearson of yet another friend; Macdonell had formed one of the little group who had provided the original guarantee fund for *Biometrika* and he had acted until the end as an assistant editor. "Few abler proof readers," Pearson said, "can be found than a Scotsman trained in Oxford, especially if he has graduated in science, and tempered his science with modern European literature as a hobby." Macdonell's patient labour, his sound advice and lovable disposition had assured him a welcome place in the Biometric Laboratory during the three or four years which he spent there between a business career in India and London and his retirement to Aberdeen. There he had held

* A little later several warm friends of biometry, among whom Mrs W. F. R. Weldon was a most generous contributor, came to the rescue of this journal and helped it over a very difficult war and post-war period.

a part-time lectureship in biometry at the University, a post since filled by another Scotsman of strong personality, J. F. Tocher.

In the war years the issue of *Biometrika* was inevitably slowed down, but the quality was well maintained. "My pride as long as I can," Pearson wrote, "is to show no sign of war in the journal, i.e. that it should be as well done as ever." For himself, to be absorbed in algebra and searching for some new statistical result, was to forget for a few hours the war-time troubles, and as no large scale research in applied statistics was possible, it was natural that he turned to mathematics. As a result several rather interesting contributions to statistical theory were completed in these years. In 1914, R. A. Fisher had written his important paper on the "Frequency Distribution of the Values of the Correlation Coefficient in Samples from an Indefinitely Large Population*." This paper verified mathematically "Student's" predictions of 1908 regarding the distribution of a standard deviation† and of a correlation coefficient, r, in samples from uncorrelated material‡; it also confirmed the substantial accuracy of certain approximations of Soper's for the general distribution of the correlation coefficient§. Besides deducing the exact distribution of r in samples from a normal bivariate population and suggesting that a mathematical transformation would be useful in the case of high correlations, the paper illustrated the value of that device which has played so fundamental a part in much subsequent work, the representation of a sample by a point in multiple space. Pearson, grasping the importance of some if not all the aspects of this paper, with characteristic eagerness was already early in 1915 planning to put theory into numbers. The standard deviation distribution was discussed in an Editorial note (89) following Fisher's paper. Very considerable algebraic treatment and computation were however needed to explore the nature of the distribution of r; the resulting paper, a long piece of co-operative work which had occupied the laboratory staff in free intervals between war work, appeared in May 1917 (90).

The sampling distribution of r, leading to curves of remarkably different shapes, made a strong appeal to Pearson's imagination. In its variety of form it had almost the elasticity of his own system of frequency curves; his pleasure in visual representation led him to plan the construction of the models still preserved in the Department of Statistics at University College, photographs of which were reproduced in the co-operative paper. To one for whom the conception of correlation had played so important a part, Fisher's theoretical rounding off of many earlier partial solutions might have had even more significance. But if controversy and acute differences of opinion were to follow, history, which, with time, must roll out all bitterness of conflicting personalities, will find here one of the striking links between Pearson's and Fisher's work.

Problems connected with mean-square contingency and the use of χ^2 were much in Pearson's mind at this time. Looking back in the light of new ideas which have

* *Biometrika*, x, pp. 507—521; the Part containing this paper was published in May 1915.
† *Ibid.* vi (1908), pp. 1—25. ‡ *Ibid.* pp. 302—310.
§ *Ibid.* ix (1913), pp. 91—115.

been evolved in recent years, we can see how he had pushed his methods of analysis forward to that stage of complexity where it was essential that some clarifying concept should be found to bind the whole into a simple scheme. In these papers he was still building, providing new results, many of which have been incorporated into the statistical theory of to-day, but the bricks did not altogether fit, there was some confusion in the pattern. What perhaps was needed most of all was a shift in outlook on the function of probability theory which would throw many results, so far disconnected, into a new perspective. The shift was not a large one, but it needed younger heads. A new generation of statisticians, turning fresh minds to the problem, could make the change almost unconsciously, and because they did not realise that Pearson's view-point was different, looking back to his work might say in surprise, here he was "wrong," there he did not "understand." So no doubt, too, will a younger generation of enthusiasts, twenty-five years hence, treat the statisticians of to-day*!

Two papers, (91) 1915 and (92) 1916, the latter written jointly with Andrew W. Young, dealt with the standard error of $\phi^2 = \chi^2/N$, the mean-square contingency of a two-way table; a correction of an error in the final formula was given in the paper entitled *Peccavimus!* (93) 1919. As a special case, the corrected formula provides the exact standard error of χ^2 in the case where the expected frequencies are known population values; but there appears to be a certain confusion in the basis of the work, and I am inclined to think that, from the practical point of view, the standard error obtained was not really what the writers needed to find†.

A far more interesting paper was that on Multiple and Partial Contingency, (94) 1916. Suppose that the individuals in a population fall into one or other of k categories, and that n_t and m_t are the observed and expected frequencies in the tth category $(t = 1, \ldots k)$ found in a random sample of $N = \Sigma(n_t) = \Sigma(m_t)$ individuals. Then Pearson considered the sampling distribution of

$$\chi^2 = \sum_{t=1}^{k} \left(\frac{n_t - m_t}{\sqrt{m_t}} \right)^2 = \sum_{t=1}^{k} (X_t^2), \qquad \text{(i)}$$

where q linear relations of the form

$$h_{s1} X_1 + h_{s2} X_2 + \ldots + h_{sk} X_k = H_s \ (s = 1, \ldots q) \qquad \text{(ii)}$$

hold among the k values of X_t, the h_{st} and H_s being known constants. Making use of the geometry of multiple space, he showed how the distribution of χ^2 would depend on what we should now term the number of degrees of freedom among the X_t's, namely $k-q$. The chance that in random sampling χ^2 should exceed any given value, say χ_0^2, could be obtained by entering Elderton's Table with $n' = k - q + 1$

* I find the following sentence in a letter of 1898 from Pearson to Weldon: "I was a good deal drawn by Galton's letter for it seemed to me that he was still hopelessly at sea with regard to the theory of regression, and if *he* did not follow the bearing of my January paper on the law of ancestral heredity, who in the world can I expect to?" But such is the accompaniment of progress, the young men step on from where the old men halt.

† The standard error of the expression, ϕ^2, that would ordinarily be calculated in practice was obtained several years later by T. Kondo, *Biometrika*, XXI (1929), pp. 376—428.

and $\chi^2 = \chi_0^2 - H^2$, where H^2 was a function of the constants h_{st} and H_s which would vanish in the common case where $H_s = 0$ $(s = 1, \ldots q)$.

This paper seems to me to contain the basis of a large part of our present χ^2 theory. Two further steps, however, were needed before that theory could be reached. In the first place it must be realised that this general solution involving the linear conditions (ii) could be applied, after some adjustment and approximation, to provide a solution in the case where the quantities m_t of (i) were not known *a priori*, but must be estimated from the sample. Secondly, a slightly different, perhaps a broader conception of the meaning of a probability measure in connection with a statistical test must be evolved. Neither of these steps was taken by Pearson nor, I think, at a later date were they ever altogether accepted by him as being justifiable.

Another paper of interest published in the same Part of *Biometrika* was that on the goodness of fit of regression curves (**95**), a problem for which the immediate suggestion was no doubt a paper by E. Slutsky on the same subject*. Pearson's work, which started without assuming that the array distributions were either normal or had a common standard deviation, led him by an arduous route towards the goal. His study of actual distributions had convinced him that when regression was not linear these conditions were rarely satisfied, and I fancy that he was too honest and too self-consistent to make the assumption which has opened a gate to a great field for further development of method. It is not that later work has been in any sense dishonest; it has followed a rather different line of attack, first deriving a technique based on certain simplifying assumptions and then showing that this technique will still be valid even when the initial conditions are not rigorously satisfied. In any case we can only admire the courage with which Pearson plunged towards an approximate solution through the heavy algebra that was required to deal with the variations in means, in standard deviations and in array totals. It was perhaps the obvious need to attempt some simpler solution, even if based on less general conditions, that led to R. A. Fisher's paper on the goodness of fit of regression curves, published six years later†.

Another of Pearson's contributions of 1916 was the last of "The Mathematical Contributions to the Theory of Evolution," No. XIX, a paper dealing with certain special types of his system of frequency curves. This was published, like the first two papers on the subject, in the *Philosophical Transactions of the Royal Society* (**23**).

During 1917 and 1918, beyond the completion of the co-operative paper on the correlation coefficient already referred to, Pearson published only two short statistical papers. It was the period of intensive computation on the gunnery programme, when he had working under him a staff of twenty persons, of varied

* "On the criterion of goodness of fit of the regression lines and on the best method of fitting them to the data," *Jour. Roy. Stat. Soc.* LXXVII (1914), pp. 78—84.

† "The goodness of fit of regression formulae, and the distribution of regression coefficients," *Ibid.* LXXXV (1922), pp. 597—612.

training and ability, for each of whom he must always have ready an appropriate job. "I get no spare moments now; it is anti-aircraft guns all day long, and for me most Sundays also," he wrote in June 1917.

The strain of this work began at last to tell and in April 1918, after fifteen months, Pearson felt that he must arrange for the group to be taken over by the Ministry of Munitions. For the last summer of the war only he and Miss Elderton remained in an empty laboratory, finishing up one or two special gunnery problems. Then at last came a month's holiday at Coldharbour, when he returned with delight to biometric work and to his garden, and so, as the following letter shows, shook off the insistent memory of the guns.

The Old Schoolhouse,
Coldharbour,
Nr. Dorking.
August 4, 1918.

My dear Miss Elderton,

I wonder how you are getting on with your holiday, I hope as excellently as I hitherto have done. First, I have had a real mental holiday—all the refreshment that comes from again taking up one of my old friends, the femur paper*. I have finished the hardest part of the task, chapter VII, on the individual characters; it was the longest chapter, perhaps, 80 pp. of printed matter with at least 70 tables of comparative racial material and it ought to form a good illustration for biometric work on these lines in the future. I only hope I have made no bad "howlers" to discredit it. Now I have commenced the last chapter on Primogenial Man and am about 1/3 through it so that I have great hopes of finishing the whole thing before my return. It would be so delightful to get it out and show our friends (? foe.) that the Laboratory is not dead!

To this I give 4 hours in the mornings and 3 at other times in the day and the mental change is really delightful. I don't dream guns any longer! Then after midday meal I turn very vigorously to gardening and try to console myself with Kipling's "Better men than you have started on their lives, By weeding gravel paths with broken dinner knives." I expect they had better lumbar muscles, however. I have got about 1/3 of the garden weeded and hoed but it is an endless task. The rambler roses are perfect just now and the evening primroses, but there is a lack of the ordinary smaller fry. Sweet peas were a failure and so largely were Shirley poppies. We have found perpetual spinach a great success and if you have not tried it, it is well worth doing....The potatoes and turnips are good. Peas and parsnips a hopeless failure and no signs of vegetable marrows yet. We have made a good deal of jam, but currants and gooseberries were not numerous. On the whole we have done well without any real gardening help. Then there is always exercising of the dogs to be considered. We have Hans and Meg and Dinah here, but so far have not succeeded in adding to our pedigree.

Do you remember Miss X. of New Zealand? I had a long letter from her this morning on a *printed* form in which Sir Francis and I both appear as founders of a new New Zealand Eugenics Society established this year. She asks me for a cablegram to say I approve and encloses an order for *three* words. I find the least I can say is *six*

* See p. 205 below for reference to this work.

words, i.e. "X., Hastings, New Zealand, Pearson disapproves." But why should I have to pay 4/- extra,...? It is a funny world....

May I have a line to say what you are doing? I hope you are resting.

Yours very sincerely,

KARL PEARSON.

Back at College on September 1st, he was ready to start training two or three members of a new staff. To what work he would have turned then had the war continued, I do not know. At the hour when the Armistice was signed, on November 11th, 1918, he was giving a first lesson on a Brunsviga calculating machine to a wounded New Zealander who has since become one of the leading experts on scientific computing.

The year 1919 was for all of us a year for stock-taking and adjustment. After the sudden release of tension in November 1918, we began to look round and wonder how we stood; could we go back to where we had left off in 1914? No, that was out of the question! What was lost? What remained? For Pearson there had been no war casualties of close friends or relatives, but the war years perhaps played their part less directly in shortening the lives of several of his older friends. Macdonell's death in 1916 was followed by that of Lord Parker of Waddington in 1918 and of Goring in 1919. Robert Parker had been Pearson's closest College friend, they had shared chambers in the Temple and joined in the founding of the men and women's club to which I have referred. Their careers had followed widely different paths, but the lawyer and judge had retained a warm interest in his friend's more adventurous course and had been one of the five original guarantors of *Biometrika*. Of Charles Goring I have already spoken; he was a man for whom more than any one else Pearson would have liked to find a post in his laboratories. Those four years of physical and emotional stress must have had some effect, too, on Pearson's own vitality. He had still his old power to inspire those who worked under him and the strength to push forward eagerly with his own ideas, but perhaps it was harder now for him to keep abreast of the ideas of others, or to step out from his study to defend the causes in which he believed.

In a more material sense the war period had indeed dealt a hard blow to the prospects of the Galton Laboratory. Funds which would have proved adequate to equip the new building in 1914 were now quite insufficient to meet a 300 per cent. rise in costs; the pre-war salaries of his staff were on a scale that in changed conditions ceased to provide a living wage. Nevertheless, slowly, with help from many generous friends, with the aid of a public appeal, with added contributions from University sources, the situation was mastered. The London County Council agreed to provide funds necessary for the salary of a medical officer and his assistant, a post created, alas, too late to be offered to Goring. The Medical Research Council gave an annual grant to Dr Julia Bell to enable her to carry on investigations into the inheritance of disease. The Drapers' Company continued their support. And so in October 1919 Pearson and his staff were able to occupy the first floor of the Bartlett building. By the end of that Session the whole territory

had been occupied, although parts of it were still very sparsely furnished, and the total staff of the twin Biometric and Eugenics Laboratories had risen to ten persons, a figure at which roughly it remained during the following years.

At this period Pearson was giving much time to the completion of a long-delayed monograph, "A Study of the Long Bones of the English Skeleton" (50), written jointly with Julia Bell, and to the allied papers with Adelaide G. Davin, "On the Sesamoids of the Knee-Joint" (51), (52). Pearson's original object had been to show that a precise technique of measurement, leading to biometric methods of analysis and comparison, could be developed for the long bones of the skeleton as for the skull. While circumstances had limited the scope of the research in the sense that it had finally been concerned almost entirely with the femur, it had been extended before completion in another direction. Not only was comparison made between the English femur and that of other races, but ultimately, after following back the development from recent man to palæolithic times, Pearson was drawn into a study of the femora of the Primates. Thus the Chimpanzee, the Gorilla, the Orang, the Old and New World Monkeys, the Lemuroids and Tarsius were considered. This use of the intensive study of a single bone to throw light on the development of man formed the subject of Pearson's lecture of May 14th, 1920, to the Royal Institution, entitled "Side Lights on the Evolution of Man" (96).

In working at the femur many data had been collected regarding the sesamoids of the knee-joint; with the evolutionary problem in mind and a trained biologist at hand in Miss Davin, Pearson extended his investigation to the case of many mammals, birds and reptiles and produced a paper beautifully illustrated by Miss Davin herself and by Miss Ida McLearn, who for a number of years was to carry on the Laboratory tradition of good draftsmanship. The object of the paper, it was stated, was "to suggest problems to those better equipped for studying them than the present authors, rather than to present solutions." Might not the least important of little bones, which anatomists following Galen had passed by as of no interest, contribute their own special share to the general assault on the enigma of evolution?

With interest keenly roused in such work as this, the war-time memories were receding. On June 4th, 1920, the new building was formally opened by the Minister of Health, and the Department of Applied Statistics set out afresh on a new venture.

1920–1933

When we mind labour, then, then only, we're too old.
Robert Browning.

The post-war years were not favourable to the spread of Galton's eugenic creed. Too much idealism had been poured out vainly in the battlefields of France. Men were tired by war, disillusioned with peace, restless, too much concerned with troubles of the present to be ready to plan a course aiming at distant horizons.

The war gap had made a break there, without doubt; it is likely that Pearson himself, with the world's reckless spending of its best in mind, was too conscious of the irony involved to return with any eagerness to the task of urging man on to the improvement of his breed. But if interest in Eugenics was for the moment set aside, there was a growing call for the use of statistical method. The Medical Research Council invited Pearson to be chairman of their Advisory Committee on Statistics*; it was a post that he felt unable to accept because of the large amount of work that must be done in his own Department, but the recognition which it implied of his many years of effort was heartening. In the Sessions 1920–22 the lecture courses on the theory of statistics at University College were very well attended. The audience was drawn from many directions; it included several members of the staff of the Industrial Fatigue Research Board, an Indian Civil Servant, several Cambridge mathematical graduates, a number of Americans including two professors on sabbatical leave, and finally five undergraduate students working for the new Honours Degree in Statistics.

The institution of this Degree by the University in 1915, as a result of Pearson's initiative two years earlier, meant that the subject of mathematical statistics had now an official standing in the University. There followed also some change in the programme of the Department; lecture courses which in the past had been arranged somewhat informally to suit postgraduate students had now to be planned in accordance with a definite syllabus. There were main first and second year courses in statistical theory given by Pearson himself, each consisting of two one-hour lectures a week with several hours of practical work and, in addition, auxiliary courses taken by his assistants on probability, on interpolation and quadrature and on periodogram analysis.

Pearson's feelings regarding the introduction of an undergraduate element were mixed. He would have welcomed a steady supply of three or four good students a year; some of these might remain as research workers, the rest he felt could be sure of finding posts outside. He regretted the refusal of the University to grant an Intermediate Examination in Statistics, which prevented students being brought into touch with an interesting subject at an early stage in their careers. Nevertheless, when some years later a very young generation full of youthful spirit romped down his polished stairs or trundled each other round the class-room in a small hand-cart meant for carrying books, he felt justly indignant. He often had cause, too, to grudge the time which his staff must give in teaching students how to handle their practical work and, in particular, in finding out their mistakes. He was anxious that the Department should not be swamped by undergraduates; it must remain a "research," not a "teaching," department.

The written record of Pearson's lectures remains only in the note-books of his students; his was not a style that would have led easily to a text-book, though

* This was a second public recognition by the medical profession of the position which Pearson occupied, although outside its ranks; in December 1919 he had been elected an honorary fellow of the Royal Society of Medicine.

many tried to persuade him to write one. As his old engineering student, whom I have quoted, said*: "K. P.'s methods were secondary to his personality—that was the key to his success, in the keenness and interest his students took in all his classes." I believe, however, that it is worth putting on record (in Appendix IV) a summary of the subjects covered in his two lecture courses on the theory of statistics given in the Session 1921–22, for which my own lecture notes taken at the time are available. In discussing these lectures, their manner of delivery and the subject-matter they contained, I have made free use of the impressions with which other students of that and somewhat later periods have kindly supplied me†.

The new student, accustomed to lectures elsewhere, was perhaps surprised that we started with reference to no text-books; but as the course progressed we were referred to volumes of *Biometrika*, to the *Tables for Statisticians* and to papers in the Royal Society publications. We heard much of Galton, something of Yule, of Edgeworth and of Sheppard, but of other writers little, and that sometimes with a warning. We were told of the sins of many people: of the compilers of Government Statistics; of the writers on the Theory of Errors, who had illustrated the Normal Law on data which in fact gave a fit of 1000 to 1 against; of the astronomers, the psychologists and the chemists who used hopelessly small samples; of the anthropologists who would not recognise the value of biometric methodology. But if Pearson spoke critically as one having authority, he would humorously admit his own errors and he would give us lavishly, what after all we really wanted, his own views on things; he inspired us by showing his creative mind at work, throwing out at the same time frequent hints of problems that needed still to be solved. Several of these were, in fact, taken up by his listeners.

He usually took some pains after dinner on the evening before to prepare his morning lecture; he would come into the class-room with these notes, put them on the table and then proceed to lecture for an hour without ever, save on very rare occasions, referring to them; even then it was probably only to see what was the next subject with which he proposed to deal. During the hour, the board would be filled several times over with algebra and diagrams, all of which it was possible for a keen student to take down in notes in a form which was afterwards readable and lucid. He would plunge sometimes with seeming delight into a piece of long and heavy algebra, retaining all the interest of his class while doing so and imparting a certain spirit of excitement and wonder as the complexities were untied into a neat solution. He had the lecturer's gift of appearing to discover a result for the first time himself. If, as happened now and then, he saw that his equations were coming out wrong, he would pause, regarding the board reflectively, with the remark "Let's be cautious, let's be cautious," and he showed evident satisfaction when the correct result was obtained.

* Part I of this memoir, p. 208.

† E.g. Mr C. H. L. Brown, Dr J. O. Irwin, Mr Frank Sandon, Miss Brenda Stoessiger (now Mrs Clapham).

He was sensitive to the response of his audience. Many will remember how he watched anxiously to see if they had followed some special point that he had made, and the pleasure which he showed on finding that they had grasped it. Speaking of one of his foreign professors he would say, "I like old ——, he always beams back at me when I make a nice point." While he was sometimes temporarily annoyed at being interrupted in the middle of writing a formula, he welcomed intelligent questions and blamed his class for lack of attention if they allowed an algebraic slip of his to remain unnoticed for several lines.

Until the last two or three years he kept in touch with every student in the Department, making it a practice to go round the laboratories at least once a day and say a few words to each about their class work or research. Such visits would extend sometimes to half an hour or an hour, K.P. sitting down with pencil or pen to illustrate his remarks or to look for a puzzling error. He was sympathetic to the difficulties and criticisms of his younger research workers and students; sometimes he would spend much time in a subsequent lecture discussing more fully a question on which they had expressed doubt. As I have mentioned in an earlier section, it was, however, sometimes more difficult to thrash out a difference of opinion with him at a later stage of one's career.

It will be interesting, I think, to give some attention to the subject-matter of the first and second year lecture courses with which these recollections are associated, because we shall find here evidence of the stage to which by 1921 the mathematical theory of statistics had been built up in England, largely through Pearson's own efforts. Turning to the summary in Appendix IV, it will be seen that the course began with an outline of the conception of correlation. I find on page 1 of my Notes the following statement, which was probably taken down fairly closely from Pearson's words:

"The purpose of the mathematical theory of statistics is to deal with the relationship between 2 or more variable quantities, without assuming that one is a single-valued mathematical function of the rest. The statistician does not think that a certain x will produce a single-valued y; not a causative relation but a correlation. The relationship between x and y will be somewhere within a zone and we have to work out the probability that the point (x, y) will lie in different parts of that zone. The physicist is limited and shrinks the zone into a line. Our treatment will fit all the vagueness of biology, sociology, etc. A very wide science."

It was this idea of correlation, first drawn from Galton's *Natural Inheritance*, which stood for Pearson as the fundamental illuminating conception of the statistical calculus. Just as another man might have taken as a leading *motif* the application of the abstract theory of probability to the solution of problems in the perceptual world, or the assignment of total variation into parts attributable to various causes, so it always seems to me Pearson took this concept of correlation. He referred to it often in his written work; thus we find in the new fifth chapter

on "Contingency and Correlation—the Insufficiency of Causation" added to the third edition of *The Grammar of Science* ((12) p. 170) the following paragraph:

"As a method of predicting the experience *likely* in the future from the experience of the past, the summary of the past expressed by function or under the category of causation has done immense service. But it is incomplete in itself, for it gives no measure of the variation in experience, and it has trammelled the human mind, because it has led to a conceptual limit dominating actual experience. We have tried to subsume all things under a perfectly inelastic category of cause and effect. It has led to our disregarding the fundamental truth that nothing in the universe repeats itself; we cannot classify by sameness, but only by likeness. Resemblance connotes variation, and variation marks limited not absolute contingency. How often, when a new phenomenon has been observed, do we hear the question asked: What is the cause of it? A question which it may be absolutely impossible to answer, whereas the question: To what degree are other phenomena associated with it? may admit of easy solution, and result in invaluable knowledge."

Those who attended his lectures will remember that characteristic correlation belt which he drew on the blackboard and the evident pleasure with which he filled its broad zone with dots, each tap of his chalk hammering, as it were, a fresh nail in the coffin of causation.

Returning to the details of the course of 1921, it will be seen that he passed from correlation and contingency through linear regression to non-linear regression, and then turned to consider the form of the variation within each array about the regression line. There was a certain grandeur in this planning which led to the consideration of variation as a feature of correlation; it was something which captured the imagination in a way which the more usual scheme of starting with the description of a single variable and proceeding to study the relationship between two and then three etc. does not. The early introduction of orthogonal polynomials did not, however, make the going easy for undergraduate students.

About sixteen lectures were devoted to Pearson's system of frequency curves. It was a part of the subject with which he particularly enjoyed dealing and he was able to convey this interest to his audience. As one of his post-war students writes:

"If I had to single out any part of the course which has remained most of all in my memory, it would be the wonderful *unity* of the system of frequency curves and the logical development of the subject, each curve being derived from the fundamental differential equation according to the assumptions made regarding the values of the constants in that equation....K.P.'s own enjoyment at seeing a set of data accurately represented by a frequency curve was a pleasure to watch."

In laying considerable emphasis on fitting frequency curves, Pearson was concerned not only to provide his students with a useful tool, but to give them a training in method and accuracy; the work gave an opportunity for practice in the use of tables, the application of interpolation and quadrature formulae and even

of draftsmanship, since, at any rate in the earlier days, students plotted their curves and were shown the use of a spline and a planimeter.

There was one aspect of Pearson's approach to his frequency curves on which I have never been quite clear. The fundamental differential equation could be derived by considering the ratio of the slope to ordinate in a hypergeometric series, or, as a particular case of this, in the binomial series. He had seemed, at any rate at one time, to feel that there was some physical link underlying this relation which gave a special significance to the resulting curves. In later years he would still refer to this aspect of the matter, but he laid more stress on their proved usefulness as graduation formulae. Their value in another field, that of sampling theory, was first shown in "Student's" paper of 1908* in which a Type III curve with the correct moments was used to represent the unknown true distribution of the variance in samples from a normal population. The system has been used increasingly in recent years for similar purposes. Finally R. A. Fisher's work has shown how the mathematical forms of the Type I, II, III, VI and VII curves give the exact sampling distribution of various criteria used in the analysis of variance.

When dealing with the problem of graduation there are sometimes undoubted difficulties in fitting the curves and there may be genuine differences of opinion as to the most serviceable method to employ in this process; but a perversity which denies both the practical utility and theoretical interest of the system can only result in a serious curtailment of useful tools in the statistician's workshop.

After dealing with frequency curves, Pearson approached the problem of statistical inference, "the fundamental problem of statistics being," he said, "to predict from the past what will happen in the future." He spoke much as he had written in the latter half of chapter IV of *The Grammar of Science* and in the Gresham Lectures, and he added the theoretical work on the extension of Bayes' Theorem that he had recently published (97). This paper, as Pearson afterwards recognised (98), contained a curious oversight, two distinctly different frequency functions being assumed identical, and an unduly simple result obtained in consequence. Perhaps it was due to a temporary lack of clearness in thought, a fault to which, I suppose, all of us succumb at times! In any case in this particular instance the error acted as a stimulant on various members of his class; we discussed and argued and experimented with it, and from the new ideas which were set in train we were the gainers.

The latter part of the first-year course was concerned with the study of probable or standard errors. The line of approach to this subject had been determined by the types of problem with which biometry had been faced in the past; it had been necessary to compare populations from which large samples could generally be obtained, or to determine the effect of selection of one character upon the variability or intercorrelation of other characters. With large samples, first approximations to standard errors were adequate and these could often be obtained by the very

* *Biometrika*, VI, pp. 1—25.

serviceable method of expansion of the variable measure in terms of "statistical differentials." As far as possible, Pearson avoided any initial assumption about the form of the observed variates. Thus the standard error of a squared standard deviation was first expressed in terms of the β_2 of the sampled population; as a special case, β_2 was set equal to 3.

A division was set between large-sample and small-sample theory, where later work has tended to emphasise continuity. For example, I find the following sentence in the 42nd lecture: "It is at this point that the whole theory of small samples diverges from our subject, and what may be said in the near future as to the probable errors of frequency constants will be based upon the assumption that our original population is large and that the sample is not small." It is interesting to note, too, that while Fisher's derivation in n-dimensioned space of the distribution of the squared standard deviation, s^2, was given, the result was used to examine how quickly the approximate formula for the standard error of s might be employed and how soon the sampling distribution might be taken as effectively normal, rather than to consider applications in connection with small samples. Again "Student's" distribution of z was not discussed in these 1921–22 lectures*, because Pearson felt no interest in work based on small numbers. No doubt if he had been faced earlier in life with the type of problem that *must* be tackled in an experimental brewery or in an agricultural research station his views on small samples would have been very different.

The second-year course dealt with: (1) Partial and multiple correlation and the application of this theory in a number of directions, including problems of ancestral heredity. (2) The χ^2 test of goodness of fit, which was derived from the multiple correlation distribution. (3) The simple principles of the Mendelian theory and the resulting correlations of heredity to be expected in a population mating at random. (4) The development of probable error theory in the case of a bivariate distribution. (5) Methods of estimating correlation from variables classed in broad qualitative categories. (6) A number of miscellaneous problems such as the variate difference method, Galton's individual difference problem, etc.

Throughout both courses papers containing illustrative examples to be worked out were issued in connection with each piece of theory. Possibly these examples involved the students in too much heavy calculating labour, but the training was designed to make quick and reliable computers, unafraid of any task and familiar with the use of a wide variety of methods.

Looking back now at these courses after fifteen years, one is struck inevitably by the big developments in statistical theory that have intervened. Yet I believe that those of us who received Pearson's training, and have since owed a great deal also to the stimulus of R. A. Fisher's new ideas, are aware of an essential continuity which runs through the central line of English mathematical statistics. As I have suggested above, does not the greatest change lie perhaps in the interpretation of

* This remark does not apply to Pearson's lectures in later years.

the meaning of probability? In applying the mathematical theory of probability, involving abstract concepts, to the interpretation of observational data, there can be no unique solution, no right or wrong. Within the field of mathematical theory there can be errors, but how we use this conceptual model to help in determining our decisions in the world of experience must remain to some extent a question of individual choice. Probably it was a failure both on the part of Pearson and of his critics to recognise this distinction which led to much controversy on the use of χ^2 and other matters.

A single observed event, whether the frequency of individuals in a class, the value of a mean $\bar{x}$ or of a measure of discrepancy such as χ^2, cannot provide a measure of probability until we have defined the set of objects, or what has been termed the fundamental probability set, to which it belongs. And here there may be considerable variety of opinion as to the most appropriate set. It is worth while trying to throw some light on this point by considering a simple concrete example, because we are, I think, concerned with two distinct views, one of which seems now on the whole to be regarded as more useful than the other.

Consider the case of a statistical test to determine whether the population mean of a variable x, known to be normally distributed, can have a specified value ξ, when a random sample of n individuals gives a mean $\bar{x}$ and standard deviation s. Pearson, following what may be described as the classical tradition, considered the fundamental criterion to be

$$\frac{(\bar{x}-\xi)\sqrt{n}}{\sigma} \qquad \text{(i)}$$

where σ was the population standard deviation. If σ were unknown (remembering that in large samples s would be close to σ), he considered that he could form his opinion as to the likelihood that the population mean was ξ by obtaining from the tables of the normal probability integral an answer to the following hypothetical question: How often could as large or a larger value of

$$\frac{(\bar{x}-\xi)\sqrt{n}}{s} \qquad \text{(ii)}$$

be expected to occur in random sampling from a population with mean$=\xi$ and standard deviation$=s$? It was the set of possible samples from this population which formed the fundamental probability set.

The new approach was to regard the value of the ratio (ii), calculated from the sample, as belonging to a different probability set, namely the set of values that would be generated if we took repeated random samples from normal populations with mean at ξ and the same or different σ's, and inserted each time the observed sample s into the ratio. The old approach led to the tables of the normal distribution, the new to those of "Student."

It will be seen that in both cases the set of objects to which the single observed ratio is referred is really conceptual, not experiential; yet many of us feel now intuitively that the second set is the more appropriate, largely perhaps because of

the direct correspondence between the probability measure found from the tables and the risk of error which will follow in the statistician's long-run experience, if he calculates ratio (ii) and rejects the hypothesis he is testing at a given probability level. I believe a closely similar change of approach can be traced in the use of many others of the tests with which Pearson originally provided us, e.g. that of χ^2. It is a change which has seemed to a new generation to throw a wealth of illumination on to many problems, to bring results hitherto disconnected into a new perspective. We felt that we had fallen on one of those simple and clarifying laws of thought which Pearson had described for us in his *Grammar of Science*. It is quite certain that he did not himself feel that simplification; nor was he alone in this. He had trained his mind to regard probability in a different way and at the age of 65 or 70 the new ideas, often somewhat obscurely presented, were not easily grasped or if grasped did not appear very profitable. We cannot say he was wrong nor that his opponents were right; it well may happen that twenty or thirty years hence our own views of to-day will have succumbed to a fresh outlook or even to something like the old outlook. But where perhaps he was at fault was in a failure to recognise that a younger generation was as genuinely chasing along new lines of thought as he had been himself in the '90's. The old watch-dog should have let them pass!

I have spent some time on this discussion of statistical theory because those who have studied the pages of *Biometrika* since the war will realise the important part that the development of theory still held in Pearson's thoughts. He was far from satisfied with the position he had reached. He saw, he thought, how further advances could be made, but knew that his mathematics were not now adequate for the task. "I wish I were young again, I wish I were young," a friend remembers him saying, standing one day on his hearth rug, smoking an after lunch cigarette and looking past her along the pathway he could see, with that characteristic upward tilt of the head. Up to that last suggestive paper of December 1933 (99), on what he termed the P_{λ_0} test, he was continually coming back to questions of theory. These papers are full of fresh ideas, but to study them with profit the reader may need to have some understanding and sympathy with an approach which differs from his own.

If we pass in review the many enterprises freshly entered into and satisfactorily completed under Pearson's direction between the official opening of the new Galton Laboratory in 1920 and his retirement in 1933, it is difficult to realise that this period covered the interval between his 64th and 77th years. In this connection he would certainly himself have reminded us of Galton, who published his *Natural Inheritance* at the age of 67 and subsequently propounded the science of eugenics. But Galton was to a large extent writing and thinking in the peace of his own study, while Pearson had to cope from day to day with the active direction of a research and teaching department. I shall not attempt any detailed description of these thirteen years, partly because they are still too near in time and partly because

it is not easy for one who was a member of the staff to place in proper perspective the department's many activities*. It must suffice to give some account of the lines of work most clearly stamped with Pearson's own individuality; among these the following are of particular interest: (i) *The Tracts for Computers* and Statistical Tables, (ii) Dog Breeding and the Animal House, (iii) the Anthropometric Laboratory, (iv) Craniometry, (v) the Museum, (vi) *The Annals of Eugenics*.

(i) *The Tracts for Computers and Statistical Tables.* The origin and objective of the series of *Tracts for Computers* is given in the Editor's prefatory note to No. I (100) as follows:

"During the course of the past five years the Department of Applied Statistics in the University of London has carried out a great deal of computing work of one kind or another bearing on special war problems of a physical character. Its members have been struck by the absence of any simple text-book for the use of computers and still more by the absence of obviously necessary auxiliary tables. The present series of *Tracts for Computers* will endeavour to fill this gap as far as it lies in our power. It will not concern itself with the higher mathematical theory, but solely with the practical difficulties of the computer, or rather such difficulties as we have met with in our own experience. The first tract will be followed not only by others containing recently computed tables or by the re-publication of old tables at present very inaccessible, but by tracts dealing with interpolation, quadrature, mechanical integration, calculating machines, tabling machines, and bibliographies of memoirs and of tables having special value to the practical computer."

The series ultimately contained such widely different works as Henderson's Descriptive Catalogue of Mathematical Tables (only partly completed), Tippett's Random Sampling Numbers and A. J. Thompson's monumental 20-figure Logarithms. I think that Pearson felt that on each generation of mathematical workers there lay a duty to contribute a share in the gradual building up of a corpus of accurate mathematical tables. In a long life he played his part nobly with his own Brunsviga; it was his custom, too, to put each research worker on to a piece of table computing at some point of his training. The successful achievement of an exacting though dull piece of work was in his view a good test of character. *The Tables of the Incomplete Gamma-Function* (101) 1922, *The Tables for Statisticians and Biometricians*, Part II (102) 1931, and *The Tables of the Incomplete Beta-Function* (103) 1933, were the most important results of many years of long co-operative labour.

(ii) *Dog Breeding and the Animal House.* When at last, in 1922, the Department gained possession of an Animal House, it was too late for Pearson himself to plan any ambitious new biometric research. Indeed the building itself, an old cabman's house overrun with wild mice and shadowed by other buildings in what had once been a mews at the back of the College, was but a makeshift affair until funds were supplied for its rebuilding in 1930. It did, however, make it possible to carry on under easier conditions the dog-breeding experiment. The animals which before had

* In the five years 1925–29 alone, 130 original papers appeared from the Department of Applied Statistics; of these, 36 bear Pearson's name as author or part author, but he took a hand in the writing up for press of many more.

been placed out with various persons in kennels often far from London, could now be kept together under observation. It added also a pleasing variety to our work; we could go over after lunch and hold a puppy while the Professor measured its nose and head; or in more strenuous fashion, when the Animal Boy was on holiday, scrub pens, cut up meat and exercise the dogs in turn in the yard. Black, white, red, yellow and piebald, they were a goodly collection of young creatures, although in later years, perhaps from too little sun and too much inbreeding, they became less robust and harder to breed from.

Almost from the very beginning of the experiment in 1908 Pearson had kept some of the dogs himself at home and many litters were born and weaned at Hampstead, at holiday houses and at Coldharbour. Now this domestic burden could be ended, but two or more of the older animals were always there as old friends about the house. How many of us can remember one or other of that long succession! Tong, of the foundation stock, Ling, her son, who as a puppy had lived with Galton and who was afraid of no man or dog however large, Choo, Hans and Grethel, Donach Ruadh, Meg and her son Ben, Topsy, Kar and Eld, Shagpat and Gemima. To their master, their characters were as interesting and varied as their coats; and if at times they gave cause for anxiety when their barking interrupted work, when they strayed into the road, when they ran on the garden beds and, if science demanded, when they failed to propagate their kind, nevertheless they were a very real source of pleasure.

(iii) *The Anthropometric Laboratory.* A generous gift from a member of the staff made it possible to start the equipment of this Laboratory in 1921. Its first purpose was to obtain records from tests on the College students. The sight, the hearing, the judgment, the mental agility, the strength, the physical measurements in a great variety of forms were taken and a wide record of faculties preserved. It was hoped also to associate these results with University achievements. The work was in charge of Dr Percy Stocks, but when in full session the help of all the staff was needed, each taking charge of two or three tests. Until trouble with his eyesight prevented him, Pearson was responsible for the head measurements. After the novelty had worn off we found it a heavy job and in time there was difficulty in obtaining an adequate supply of students. Perhaps we had too many tests, but Pearson felt that one could never be sure of what was most important in a research until a full record of data was available for analysis; better to discard then, than to fail because of inadequate material. I have often admired the patience with which Dr Stocks collected his somewhat reluctant band of workers and spent many lunch-time hours himself bearing the brunt of the labour.

(iv) *Craniometry.* With Pearson's contribution to craniometry I am not fully competent to deal. I have given above, however, some indication of what I believe were the objectives of the long-time research that he planned: the accurate description of racial characters within each group and, as more material became available, a critical study of the relationships between groups. The Department became a store-

house for more and more series of skulls, some supplied from Egypt by Flinders Petrie, others obtained from the London area when new building opened out an old burial ground or plague pit. The collection of about 7000 skulls which Pearson left at his death is something of unusual value.

In the years after the war he was to find an able and sympathetic collaborator in Dr G. M. Morant, who played an essential part in the work of the craniometric laboratory, taking most of the responsibility of training the research workers off Pearson's shoulders and cheerfully bearing much of the heavy labour of getting their papers in order for the press. Morant was also in later days responsible for photography; money for the equipment of the photographic rooms was never obtained, but he and others made good use of an old full-plate camera, a tried friend of the Biometric Laboratory for more than thirty years.

Pearson's own contributions, among which may be mentioned a paper "On the Biometric Constants of the Human Skull" (**104**), papers on the Coefficient of Racial Likeness (**105**), (**106**), an Oxford lecture on a new Cranial Coordinatograph (**107**) and a study of the individual bones of the skull (**108**), are characterised by a rare suggestiveness and breadth of view. Of some other original uses to which he turned his interest in physical anthropology I shall speak below; here it may be of interest to note that the period of this section of this memoir started with his presidency of Section H (Anthropology) of the British Association in August 1920 and ended with a lecture, his last public lecture, to the Oxford University Anthropological Society in May 1933. Further, in 1932 he was awarded, as the first foreign recipient, the Rudolf Virchow medal of the Berlin Anthropological Society.

(v) *The Museum.* In the original plan of the Francis Galton Laboratory, there was a door into the street through which on certain days the public would have been admitted to a museum bearing on the subject of Eugenics and thence enticed into an anthropometric laboratory. The authorities decided that an independent entrance to the College could not be permitted and so this plan had to be abandoned; nor, indeed, after the war were funds available to provide the staff needed to deal with the public in this way. There was, however, space provided for a large museum and this was gradually equipped with cases, through the help of many private benefactors. The range of exhibits was typical of the wide interests of the Director. In deep cases down one wall were statistical models, some dating back to his first attempt in the Gresham College lectures on Geometry to make clear the meaning of probability theory to a popular audience; others of more recent date dealt with advanced statistical theory. In revolving show cases was a collection of photographs, drawings and pedigrees, illustrating the inheritance of good and bad qualities in man; pedigrees of ability, as in the Bach, the Maclaurin and the Bernoulli families; pedigrees of defect, whether of insanity, digital deformities, congenital cataract or haemophilia; photographs of albinos, of dwarfs and of lobster-claw deformity. There was also a special section dealing with the early history of man; artefacts, prehistoric burials, casts of bones and of some of the more famous prehistoric skulls; also copies

of the reconstructions by Mascré of Brussels of certain types of early man. Other cases contained type skeletons of the Pekinese and Pomeranian dogs and of the various degrees of their crossing. There were skins of these dogs, too, and of the hares discussed in the *Albinism** whose coat colour changes with the season; paintings of albinotic eyes; colour scales and examples of various measuring instruments.

There can have been few museums that bore a closer, a more striking witness, to the activities and interests of a single mind. The sympathetic visitor who was fortunate enough to be taken round by Pearson himself did not easily forget the occasion.

(vi) *The Annals of Eugenics.* Several series of popular lectures on the work of the Biometric and Eugenics Laboratories were given after the war; for example, Pearson gave a course of 10 lectures in the spring of 1921 and other courses in which the members of the staff took part were given in 1922, 1924 and 1929. But popular interest was far less keen than before the war and it seemed that there was little prospect of success at that moment for Galton's plan of "extending the knowledge of eugenics by occasional public lectures." Pearson's last great effort towards the establishment of eugenics as a science was the founding in 1925 of a new journal, *The Annals of Eugenics.*

"The time seems fully ripe for the issue of a journal," runs the editorial Foreword, "which shall devote its pages wholly to the scientific treatment of racial problems in man. Several journals allot some of their space to original memoirs dealing with eugenics and the general problems of race hygiene. Others of a minor character spend their main energies in popular articles, book-reviews and accounts of matter published elsewhere. Our journal will differ from existing journals in that bibliographical matter will be reduced to a minimum, that no other topics than the problems of race in man will be dealt with, and that the papers published will be the work of trained scientists rather than of propagandists and dilettanti. Naturally a journal issued by the Galton Laboratory will be sympathetic to the methods of its founder summed up in the title of his Herbert Spencer Lecture 'Probability the Foundation of Eugenics.' But this does not signify that contributions dealing with heredity in man from any scientific standpoint will not be acceptable. Nevertheless the study of man is essentially a study of mass-movements and mass-changes. Selection can hardly take place in man except by selection of somatic characters, and the results of such selection can only be effective as an evolution, according to the extent to which somatic and germinal characters are correlated. The existence of such a correlation is an undoubted fact, whatever theory we may choose for its expression...."

"It may be argued that there is a science of Eugenics which is not our Eugenics, and if one might place faith in the multitude of text-books, which have adopted the name, the argument would be complete. Most of these text-books, however, have merely taken the name and nothing else from the founder; they mix a little biology with a trifle of genetics, and water the whole down with much tea-table talk on the impracticability of fundamentally improving the race of man. In such manner no great science was ever

* (49) Part II, pp. 421—442.

built up; it must have definite methods of attack—be in short a concise discipline—realise its problems and grasp how their solution can be approached. It was such a discipline that Galton foreshadowed when he claimed that probability was the foundation of Eugenics...."

"It is the aim of our journal to aid, as far as lies in its power, the oncoming of the day when we can claim that the groundwork of our science has been securely laid, and both the student's text-book and practical eugenics—eugenics applied to national problems—will then be feasible. Let us bear in mind the words of Galton written almost in the last years of his life, words not of despair, but of wise caution: 'When the desired fulness of information shall have been acquired, then and not till then, will be the fit moment to proclaim a "Jehad" or Holy War against customs and prejudices that impair the physical and moral qualities of our race.' This has been the spirit in which the Laboratory he founded has been conducted, and that will be essentially our guide in the control of this journal."

It was inevitable that at first the great majority of the contributions to *The Annals* came from inside the Department of Applied Statistics; memoirs of a kind which before had been issued in the Drapers' Company *Studies in National Deterioration* and the *Eugenics Laboratory Memoirs* were now published in the new periodical. But Pearson looked towards the future, to a time beyond the days of his own editorship, when there should be a steady expansion in the field of workers, "when every university of standing will have its professor and laboratory of Eugenics."

Of this journal he edited, with Miss E. M. Elderton's assistance, five volumes. His own chief contribution in its pages to the scientific treatment of racial problems was the long piece of statistical research on "The Problem of Alien Immigration into Great Britain, illustrated by an examination of Russian and Polish Jewish Children." This work, in which he was assisted by Miss Margaret Moul, appeared in five parts, and was not even then completed (**82**). It was based on the data collected before the war, regarding children at the Jews' Free School to which I have already referred. Three of Pearson's last public lectures, "On...the Relationship of Mind and Body" (**109**), "On a New Theory of Progressive Evolution" (**110**) and "On the Inheritance of Mental Disease" (**111**) were also issued in this journal.

Characteristically, he put the adequate presentation of his subject before the convenience of his readers. The unusually large-sized page of those first five volumes was planned to prevent any cramping of the space for tables, diagrams and skull photographs.

At the heading of an earlier section of this memoir I have set some words of Michael Angelo's, one of the mottoes on the walls of Pearson's room, which imply that the true pursuit of science can leave no man free for distractions. Nevertheless, Pearson's life was full of the practice of what may well be termed hobbies: as a professor of Applied Mathematics, biometry and astronomy had been his hobbies; so, when he became professor of Eugenics, had been research into the ancestry of Darwin and of Galton. And now, in his later years, his mind played at large in

many fields. In the speeches that he made and the toast cards with which he surprised us at the annual Galton Dinners; in the portraits and cartoons he collected for the walls of the Department; in the Museum, the models, the instruments, the busts; in his lectures on the History of Statistics; in his papers on the skulls of Robert the Bruce, George Buchanan, Henry Stewart Lord Darnley and Oliver Cromwell; in all these things we were aware of the richness of his mind.

The first of the Galton Dinners was held in 1920, the fourteenth and last in 1933; the date was as nearly as possible January 17th, the anniversary of Galton's death. They were evenings when the past and present workers and students in the laboratories collected to see and talk to their Professor and to each other, and to find themselves again in that atmosphere, undefinable but very real, which had first grown up round the little one-room biometric laboratory of the '90's. Besides "ourselves", among whom must be included a number of friends and benefactors of the Laboratories, one or two guests of distinction were welcomed on each occasion. All those who attended will remember the characteristic features of those dinners; the assembly in the Museum, K. P. greeting us by the door, Miss Elderton hurrying round, with smiles for us all, to introduce neighbours at the dinner table who were unacquainted with each other; the dinner itself in the College Refectory, the file back, two by two, along an underground passage to our own building; the roll-call in the Museum to ensure that we went upstairs in the right order to squeeze into our seats for dessert and toasts in the Common Room, or later, when numbers became too large, in one of the student laboratories.

There were at first five Toasts, afterwards increased to six: (1) In pious memory of Francis Galton, (2) In remembrance of all Benefactors, (3) In memory of the Biometric Dead, (4) The Guests, (5) The Postgraduate and Student Workers, (6) The prosperity of the Laboratory. In Pearson's speeches he would tell us of Galton, of the founders of *Biometrika*, of little incidents from his long memory of the history of biometry; of losses we had suffered by death since the last meeting; of the work carried out in the Department during the past year. There was a vein of sadness running through much of what he said, inevitable perhaps in one who looks back to recall the contemporaries or even younger friends whom he has lost. But he could deal with the present in a lighter manner. Below are some extracts from his Toasts of 1922, a year selected at random from the series.

In pious memory of our Founder.

"The present year is one of great importance in our annals. On February 16th it will be 100 years since the birth of Francis Galton. A century which deserves celebration at least equally with that of his cousin, Charles Darwin. For while the ideas that sprung from Darwin led to a reconstruction of all the biological sciences, those that sprung from Galton's inspiration are leading to a revolution in scientific logic; they must ultimately produce a renascence in every science in which statistics plays a part, and that we may truly say involves every branch of modern knowledge. But I do not desire to dwell on that side of Francis Galton to-night. I would urge rather another phase of his work, his power of exciting the affection of the most divergent men and women...."

In pious memory of Sir Herbert Bartlett and other benefactors.

"It is fitting that in this year when the donor of our new buildings has died, we should single him out for special reference and honour....

"Summing up what benefactors have done for us, we can say we have now our building and practically nearly the whole of its fittings as originally designed. We have still much to do by way of equipment. I remember the day when two machines had to serve the staff of four men and one woman, and she, our dear old Dr Lee, whom illness has kept from us to-night, went out in indignation and bought herself a machine. I regret, I cannot even say out of her stipend, for she served the Laboratory—and kept us all in proper discipline—for nearly 15 years without a penny of payment. She gave us at present rates of pay between £2000 and £3000, and possibly over 1000 persons are now using her tables and do not recognise her as a benefactor, who has saved them more than they will or can ever repay her.

"Well, we do not have two machines to five computers now-a-days, but the equipment, as we all know, is still defective...."

In pious memory of dead biometricians.*

"I will not say more this year than again couple this toast with the names of Galton, Weldon, Macdonell and Goring...."

To our guests.

"...Dr Katherine Watson..., Mr Hope-Pinker..., Dr Whitehead..., Mrs Hume Pinsent....

"And lastly I come to Sir Gregory, our Provost. I have already referred to him to-night. It is only human nature to abuse the head of the executive, if there are no towels or the windows don't get cleaned. Every failure in the government machine is of course Mr Lloyd George's fault. But when the foreigner begins to criticise our prime minister, then we are apt to see his virtues, and reply hotly that from Lenin to Poincaré we don't see a better. And looking all round the colleges and universities of the country, I don't see a better head than ours. Although having known him since I was in knickerbockers and he was in—well, in the perambulator—I may at times give way to the instinct to pummel him if he exercises his legitimate right to disagree with me.

* The following unfinished lines, written on the theme of this toast by Mrs Pearson, were found in a book by her bedside after her death in 1928:

(1)

In a quiet sheltered land, a land of Silence,
Where the spacious courts and halls of Memory spread,
Waiting till we turn and seek their counsel,
Rest our Biometric Dead.

(2)

He the Friend and Father, Francis Galton,
Man of subtle thought and simple speech,
Laboured still when weight of years was on him,
Learning, so that he again might teach.

(3)

Once again the fiery-hearted Weldon
Leaves his birds and beasts and open land,
Seeks the shaded study's dubious twilight,
Worker with our Biometric Band.

(4)

From the prison's gloom emerges Goring,
Seeks the danger-zone in Galton's camp,
Kindest leech and keenest man of science,
Holding high the Biometric Lamp.

(5)

Macdonell...[the name only written here].

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Toast Card

NINTH ANNIVERSARY DINNER

BIOMETRIC

&

GALTON LABORATORIES

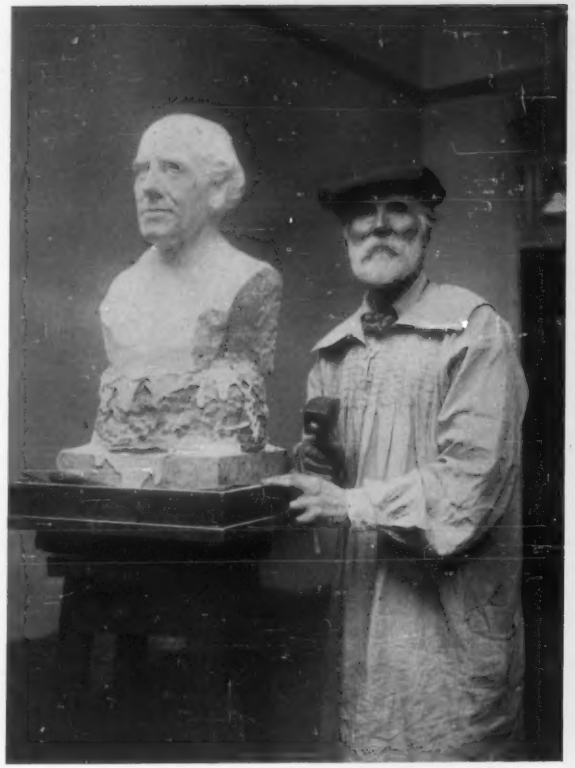
Tuesday, January 17, 1928

DESSERT IN THE LABORATORY

TOASTS

1. IN PIOUS MEMORY OF SIR FRANCIS GALTON
2. IN REMEMBRANCE OF ALL BENEFACTORS
3. IN MEMORY OF THE BIOMETRIC DEAD
4. THE GUESTS
5. THE POSTGRADUATE AND STUDENT WORKERS
6. THE PROSPERITY OF THE LABORATORY

COFFEE IN THE COMMON ROOM



HENRY RICHARD HOPE-PINKER
1849–1927

NUR DEM ERNST, DEN KEINE MÜHE BLEICHET
RAUSCHT DER WAHRHEIT TIEFVERSTECKTER BORN,
NUR DES MEISSELS SCHWEREN SCHLAG ERWEICHET
SICH DES MARMORS SPRÖDES KORN. *Schiller*

DIE HÖCHSTE WIRKUNG DES GEISTES IST, DEN GEIST HERVORZURUFEN.
Goethe

"Well, here is my last toast to our guests of to-night, and I couple with it the name of Sir Gregory Foster."

The toast-cards with which Pearson provided us at dessert were both a surprise and a delight, with their photographs, sayings or poems; in composing them his fancy would roam freely over past and present. In 1923 a pencil sketch of Francis Galton at 88 made by his niece, Mrs Ellis, no doubt present on that occasion, faced some words of Florence Nightingale's about Mr Galton and Statistics. In 1927 silhouettes of Galton at 8 and at 65 were faced by an extract from John Graunt's *Observations made from the Bills of Mortality* and backed by some lines from George Meredith. The cards of 1926 and 1928 were associated with Weldon and with Hope-Pinker; portions of these are reproduced in Plates VI and VII. The issue of the twenty-first volume of *Biometrika* was commemorated in 1930 with a photograph of the Oxford Darwin statue and some extracts from the Editorial articles written by the three founders of the Journal. In 1931, we had a portrait of H. E. Soper, "1865–1930, Mathematician, Athlete, Inventor, Statistician. Worker in the Biometric Laboratory, 1908–1921"; opposite was an extract from Matthew Arnold's *Thyrsis*:

A fugitive and gracious light he seeks,
Shy to illumine; and I seek it too.
This does not come with houses or with gold,
With place, with honour, and a flattering crew;
'Tis not in the world's market bought and sold—
But the smooth-slipping weeks
Drop by, and leave its seeker still untired;
Out of the heed of mortals he is gone,
He wends unfollowed, he must house alone;
Yet on he fares, by his own heart inspired.

Finally in 1933, at the last dinner, we found waiting in our places a card with a photograph of Miss Elderton who, thirty years before, had become assistant to Galton; this little tribute to one who had done so much to build up the spirit and tradition of the laboratories was something in which we all rejoiced. The card bore, too, in William Watson's words, a farewell message from our host and a note of questioning as to the future.

Guests of the ages, at To-morrow's door
Why shrink we? The long track behind us lies,
The lamps gleam and the music throbs before,
Bidding us enter—and I count him wise,
Who loves so well Man's noble memories,
He needs must love Man's nobler hopes yet more.

Yes, we could carry on into the future, but where was the Toast-master who could succeed K. P.? Such, among many thoughts, were passing through our minds.

We must regret now that one who felt so keenly the need to record in word or portrait the likeness of his friends was so rarely photographed and never satis-

factorily painted. Miss Footner's pencil sketch which stands as the frontispiece of the first part of this memoir was the only good portrait of post-war years; I must, however, refer to another work which resulted from the friendship with Hope-Pinker. Having used Pearson as a model for Roger Bacon before the war, the old sculptor, some ten years later, felt that he would like to try his hand at a portrait bust. He decided to make a direct cut from the marble; it was fascinating to watch the form appearing slowly out of the block, but it was a hard task for a man of over 70 to undertake. After paying many visits to the studio in West Kensington, Pearson at last persuaded Hope-Pinker to bring his marble and tools to University College where he could give him after-lunch sittings in the well-lighted Instrument Room on the second floor of the Laboratory. Even then it was a long and trying occupation, with Pearson at times fretting to be off to his own work, but his affection for the artist, who was so obviously enjoying himself at the job, carried him through some three years of sittings to the end. Once Hope-Pinker brought Tonks over to look at the work, and it was reported that the Head of the Slade School, seeing the large skylight in the room, departed muttering "Ye Gods, top-lighting like this for Science. I must see the Provost!" Poor K. P., poor Hope-Pinker, would authority step in and decree that our rather empty top floor should be handed over to the Slade? But nothing happened and at last the bust was finished and exhibited at the Academy of 1924, without the name of the subject, but bearing the following inscription:

"From life a cut direct but still my friend."

The photograph opposite p. 221 does more justice to the craftsman than to his work, but when viewed from a more favourable angle one sees that something of K. P.'s strength has been caught even if the likeness is not altogether a close one; at 75 the chiselling of the stone was a hard job for those old wrists, whatever the spirit behind them, and perhaps at the end they were not content to leave well alone.

It is not I think altogether fanciful to see a link between those hours spent in watching the sculptor shape a head with the help of eye and calliper in his three-dimensioned space, and that novel form of investigation to which Pearson gave much time in the last ten years of his professorship, the study of the relationship between the skulls, or reputed skulls, of certain famous persons and their portraits.

He did not of course regard this research in the same scientific category as his other craniometric work; it was carried out to a large extent as a hobby, but it is worth considering as such for the light it throws on one aspect of his many-sided personality. The first biometric study on these lines, involving the application of laboratory methods to historical inquiry, had been carried out by Miss M. L. Tildesley, her paper on "Sir Thomas Browne: his skull, portraits and ancestry" having been published in *Biometrika* in 1923 (112). In this work both her chief at the Royal College of Surgeons, Sir Arthur Keith, and Pearson who had supplied her craniometric training, took much interest. A year later Pearson published a short paper

16/7/24

My dear Dr Bell

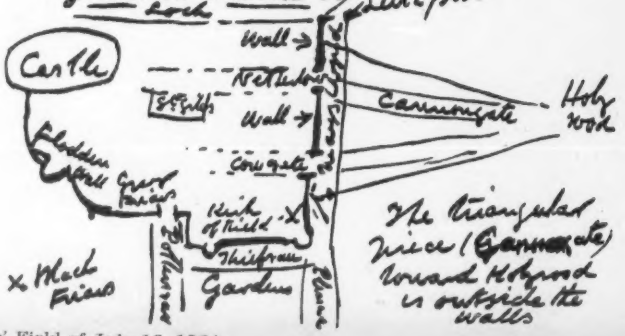
In the M.S.S Room at the Brit. Mus. the other day, they said I must go to the Reading Room to see a certain Indian Ink Drawing of Edinburgh. It occurs in the Catalogue of the Manuscript Maps Charts & Plans Vol III. 1844 under Scotland Edinburgh. and is entitled:
A view of Edinburgh from Hails. Red Hall in the foreground. Indian Ink by John Clerk. Royal XVIIX 2.64 . 2.art.64

The question is does this throw any light on the Kirk of Field & "Flodden Wall"? The latter is the southern Wall of Edinburgh. Now

Plans or views of Edinburgh are usual taken from the south in which case the Castle is on the left & the Holyrood Palace on the right, or from the north when the castle lies on the right & Holyrood on the left. This is most important as without remembering it east & west & north south may be interchanged.

South of Edinburgh and therefore in the distance in the first form of plan, and in the foreground of the second lies the Kirk of Field.

In views from the south the general scheme is



The city was bounded by a (2

is from the north you may

southern wall of Edinburgh. Now

Letter regarding Kirk o' Field of July 16, 1924.

The city was bounded by a (2)
loch on the north by the
east wall with Netherbow
& "Cowgate opening into St.
Mary's Wynd, and by the
Flodden Wall from the
South East bastion ∪ to
the Castle, which wall
goes round the Blackfriars
& Kirk of Field to the
Pollenaw Port & thence
up to the Castle. Now
the problem is whether John
Clerk's plan shows the Kirk
of Field, or the "College"
afterwards built on it. The
Kirk of Field may have
one or two towers in the centre
when with two
towers, this is before
1530, but in the 1567 plan it
is as above. If Clerk's plan
is from the north you may
possibly see the church (to
the right of St. Mary's Wynd)
with up in the distance.
What I want is a description
of anything in the neighbour-
hood of the Kirk of Field
or in the wall from bastion
to Pollenaw Port if there
be anything.
I enclose a rough sketch I
made from a map showing
old town & the modern
street names.
I am afraid it is rather
a thankless job! Most probably
Clerk's plan was nothing. There
perhaps only an orchard at the
Kirk of Field, but I did not
like to pass it by without knowing
whether it ought to be
more thoroughly investigated,
before I note up my Topographical
Section. Yours very sincerely
Karl Pearson

Letter regarding Kirk o' Field of July 16, 1924 (*continued*).

UNIV. OF MICH.

discussing the cast of a skull reputed to belong to Robert the Bruce (113); besides giving measurements, he fitted the profiles of the skull on to certain fanciful portraits and concluded that the latter were of little value.

"Even the aged dream dreams," he wrote, "and I should like to see a national monument to Bruce at Westminster, an effigy based on the skull as only a great sculptor can conceive it. But it should be the gift of Englishmen only to the united nations. It was the Norman element in Bruce, quite as much as the Celtic, which carried him and Scotland to victory at Bannockburn....A great portrait of Bruce is still possible, and if fitly executed would go a long way to justify the value of craniological study for the portraiture of national worthies."

Two years later in 1926 he devoted his Henderson Trust lecture at Edinburgh to an account of the skull and portraits of George Buchanan (114). Before this date he had, however, almost completed a much bigger task dealing with this period of Scottish history, a critical analysis of the events surrounding the murder of Darnley and the light thrown on them by a study of the skull and portraits of that unfortunate youth (115). This work of historical research was carried out by Pearson with extraordinary thoroughness and care of detail; in Plate VIII I have shown a facsimile of a letter which he wrote to Dr Julia Bell* on a point regarding the position of buildings round the Kirk o' Field, the site of Darnley's murder. The immediate problem that he had in mind was whether the markings on Darnley's skull were (*a*) due to syphilis, (*b*) produced by the explosion that was reputed to have caused his death, (*c*) caused by the action of insects or tree-roots after burial. In considering the possibility of (*b*), it was necessary to sift all the available evidence regarding the position of the lodgings where Darnley lay sick and the site where his body was found. In his final conclusions Pearson favoured the hypothesis (*a*), and believed that this fact, if it were true, threw "a flood of light on many points of those dark pages of Scottish history from Darnley's marriage to his murder." The paper was dedicated to the memory of Walter W. Seton, formerly Secretary of University College and Lecturer on Scottish History. How great is the satisfaction when at last we can make an honourable peace with our foes! For many years Seton, in his official capacity as Secretary, and Pearson, an ever-ready champion of freedom against authority, had been again and again at loggerheads. But at last, from their discussions of Mary Stewart over the lunch table, these two widely differing personalities found a bond of interest which broke the barrier. To both, Mary was of all the Stewarts "the most generous, the most cultivated and the most liberal in religion," and both were in agreement that the personal tragedy of her death "was outstripped by a greater tragedy, the strangling of the growth of a national culture and a national spirit by the insatiable greed of rival Tuchuns†." In Moray, Archbishop Hamilton, Bothwell,

* Pearson's habit of retiring to the country to write meant frequent letters of this kind full of queries. Of all his staff he relied most I think for help in this connection on Miss Bell, whose accuracy and skill in following up difficult points had already made her an ideal contributor to *The Treasury of Human Inheritance*.

† I quote from the concluding remarks of Pearson's paper.

Lennox and Morton, even in Elizabeth, Seton and Pearson could now in common find the villains in the piece.

Six years later Pearson returned to this same form of historical research in his joint study with G. M. Morant of the Wilkinson Cromwell head (116).

In following a link between Hope-Pinker, the sculptor, and Pearson's work on portraiture and skulls, I have run on too quickly in time. Before approaching the subject of Darnley, in fact as soon as he had organised the working of his post-war laboratories, Pearson's mind naturally turned back to the unfinished *Life of Francis Galton*. In 1922, the centenary of Galton's birth, he had written a short essay on Galton and his work, published in the *Questions of the Day and of the Fray* Series (117). With this essay we may well associate an appreciation of Charles Darwin, delivered in 1923 as one of a series of London County Council lectures to teachers on "Master Minds of Science," and later published (118). The lecturer gave a charming sketch of Darwin's character and emphasised the effect which *The Origin of Species* had in freeing modern science from the shackles of superstition.

> "I have told you," Pearson said, "that I am young enough to have escaped practically the dogmatic teaching in childhood which placed in bondage the minds of the generations preceding Darwin, and Huxley and Galton. But I am old enough to remember the battles of the sixties and seventies, and the joy we young men then felt when we saw that wretched date B.C. 4004, replaced by a long vista of millions of years of development. Just as much as the older men we looked upon Charles Darwin as our deliverer, the man who had given a new meaning to our life and to the world we inhabited."

The completion of the Galton *Life* was impossible without funds; luckily in 1922 a generous gift from Mr Lewis Haslam, M.P., an old schoolfellow of Pearson's, enabled him to face the difficulties of a second volume. This, the fruit of several vacations' labours, was completed and published in 1924. It dealt with the researches of Galton's middle life in anthropology and heredity, in psychology, in photography and finally his earlier inquiries in the field of statistics.

Pearson was fascinated by the suggestiveness of much of Galton's work belonging to this period; he had studied few of these earlier papers until he met them now, forty years later, in his capacity of biographer. He found many ideas that he would have liked to have been young enough to follow up; applications, for example, of photography in determining types by composite portraiture, in analysing expression and change in the human countenance, in measuring resemblance. But what it was now too late to do himself he hoped that others might some day attempt, if he could set on record here in one book, a résumé of Galton's scattered work, together with an account of his instruments and apparatus, adding as his own contribution the explanatory comment and sympathetic criticism which his personal contact with Galton made possible. There was again to be a halt of some years before the final volume was taken in hand.

During this period, 1923–1929, Pearson was giving a series of lectures on the History of Statistics. It was an extraordinarily interesting course, which started

Outline of Pearson's Lectures on the History of Statistics.

17th century

John Graunt 1620—1674

John Graunt and Sir William Petty 1623—1687

Edmund Halley 1656—1742
Caspar Neumann 1648—1715

Gregory King 1656—1714
Charles Davenant 1648—1712

18th century

Abraham de Moivre 1667—1754
Thomas Simpson 1710—1761
W. Kersseboom *c.* 1700—*c.* 1760
George Louis Le Clerc Buffon 1707—1788
Antoine Déparcieux 1705—1768

Colin Maclaurin 1698—1746
James Bernoulli 1654—1705
John Bernoulli 1667—1748
The Newton-Leibnitz Controversy
Leonhard Euler 1707—1783

John Arbuthnot 1667—1735
William Assheton 1641—1711
Assurance Societies founded:

Life:		Fire:	
Mercers'	1698	Hand in Hand	1696
Amicable	1706	Sun	1706
Union	1714		
Lloyds	1716		
Westminster	1717		
etc.			

William Derham 1657—1735
Bernard Nieuwentijt 1654—1718
Johann Peter Süssmilch 1707—1767

Nicholas Struyck 1687—1769

Thomas Bayes *c.* 1702—1761
Richard Price 1723—1791

French Writers 1750—1800

Marie Jean Antoine Nicolas Caritat de Condorcet 1743—1794
Jean le Rond D'Alembert 1717—1783
Joseph Louis Lagrange 1736—1813
Pierre Simon Laplace 1749—1827

The scheme set out above shows roughly the lines which Pearson followed from John Graunt to Laplace.

with an account of John Graunt and continued, with several interruptions when the lecturer had not time for the necessary preparation, until the period of Laplace had been reached. In the table on page 225 is printed a rough scheme showing the order in which he treated the most important contributors to Statistics in several descending lines. Pearson used to say that he believed that a University teacher ought to give every year one new course on a subject which he had not prepared for lecturing before; only so would he prevent himself from becoming stale. His own contribution to this ideal was certainly given lavishly. With his usual thoroughness he went in every case to the original source for his material; he told us not only of the writings of the individuals selected for discussion, but something of their life history and personality, and to this he added his own comments on the influence which contemporary events had upon them and on the significance of their contributions to the history of the subject. As these lectures were written out by him in very full detail, it is hoped that it will be possible shortly to arrange for their publication.

In 1923 Pearson's eyesight had begun to fail; although this only curtailed certain aspects of his work, it was naturally a cause of much depression. A successful operation for cataract in the summer of 1926, however, removed the fear that his days of active work would be prematurely cut short. But in the same year which eased this tension another blow was to strike him; his wife was laid up with a long illness which only ended with her death in March 1928.

Maria Sharpe sprang, like her husband, from a stock of Dissenters; her family had been notable for its wide interests and independence of thought. William Sharpe, her father, and his brothers had been early brought into touch with their uncle Samuel Rogers, the poet, whose hospitable table had been a centre of literary and artistic fashion during the first half of the nineteenth century. Among her uncles were Samuel Sharpe, the banker and Egyptologist who made and published his own translation of the Bible, Daniel the geologist and admirer of Lyell, and Sutton, the friend of Cuvier, of Stendhal and of Prosper Mérimée. Her father himself had thought of training as an architect before he turned to the law, and before his marriage had been on many holidays abroad, observing and recording with a skilful pencil the towns and countryside of Italy, Switzerland and France. Later in life, with the help of the books in his library, with many illustrations of the old masters and of the leading artists of the day, he was able to convey to his two sons and six daughters much of his own enthusiasm for the reasoned study of literature and art. The scientific renaissance which followed the publication of *The Origin of Species* had its influence, too, on the minds of the young Sharpes as on those of so many of their generation; the message came to them in a variety of ways, in readings of Darwin's *Descent of Man*, in lectures by Huxley on Evolution, by Carey Foster on Electricity, even in "talks to Ladies" on Human Physiology by Elizabeth Garrett Anderson, whose plain speaking shocked some of the elder generation.

To pass from an appreciation of literary and artistic values to a thirst for scientific knowledge involved no big step for minds searching to find in things an

ideal of unity. As Maria Sharpe had written in an article on "Henrik Ibsen; his Men and Women," published in *The Westminster Review* in June 1889:

"We are beginning nowadays to realize the oneness of the laws of life, and we know that in the future the man of science, the man of religion, the moralist, and the social philosopher, equally with the poet and the painter, will take a place in the brotherhood of artists, between whom there is no antagonism, who work by means of observation, selection and imaginative creation to help mankind to make itself."

As I have already mentioned, Maria Sharpe and Karl Pearson had first been brought into touch over the work of the small club whose object had been to encourage scientific investigation of all that concerned "the mutual position and relation of men and women," in past and present. When, soon after their marriage in 1890, Pearson had joined forces first with Weldon and afterwards with Galton, and the science of biometry was born, the wife with such an outlook was able to watch with whole-hearted sympathy her husband's efforts at "observation...and imaginative creation." She was no mathematician and made no attempt to follow the details of his scientific work, but she could appreciate the broad outline of objectives and methods. She saw, too, something of the romantic aspect of those early days of biometry, with the small group of workers fighting for ideals against a critical opposition. There is a long Lay in the metre of Macaulay, but touched by a strong sense of humour, which she wrote after the 1903 holiday meetings of biometricians at Peppard on the Chilterns. I shall quote the two verses in which she referred to the position of Galton's niece, Miss Eva Biggs, of herself and of Mrs Weldon, whose common duty was to guard the peace of the inner circle of biometric workers.

"And round this *inner* circle of learning high and deep,
An *outer* circle ever does watch unceasing keep,
The agile niece, our artist, and one the 'buffer-state'
Who three rampaging urchins doth carefully abate.

"One too, the model hostess, and friend to all of these,
Who has so oft presided at 'Biometric Teas,'
And yet when wheels are driving, and work is to the fore,
Will sort and count and cipher, no inner circler more."

In many ways through her married life, Maria Sharpe Pearson had played nobly the part of the "buffer-state"; with children, with household troubles, with worried laboratory staff, with barking dogs and litters of puppies. To her love and admiration for her husband she added a certain power of detachment; from the days when she was club secretary in the '80's she had seen how his uncompromising search on the track of knowledge and truth had brought him into conflict with other minds and she could give helpful counsel which had eased many situations. The traditions which surrounded her own upbringing had provided her with much of the same philosophy of life as Pearson; to her, also, the moral was the social, the

a-moral the anti-social. Above all she could understand that "creed of life" which makes a man "serve science from love as men in great religious epochs have served the Church," and she knew well how much it was worth while to put aside for its sake.

It was with this companionship that the final link was severed on March 30th, 1928.

With the house and countryside at Coldharbour too full of memories of the past twelve years, Pearson spent the summer vacation of 1928 with his daughters in the Black Forest. Here he was at work on the third and last volume of the *Life of Galton*. The following letter gives some account of their day's programme:

Gasthaus zum Ochsen,
Saig.
August 26, 1928.

My dear Dr Bell,

It was very pleasant last evening to receive a letter from you, and hear of your doings. I fear I cannot make compensation for the letter my wife used to write, for she had a great faculty for writing sympathetic letters. But such as I can, here it is. S—— and I have had a very quiet time, I fear dull for her as we have had little change of thought with any one, but the one or two Germans next us at meals. I have not been very good at expeditions, because I am liable to get overheated and had five days in my room with lumbago as a warning. It is absurd as one grows old to have to take so many precautions. However the weather has been most favourable, and we have been able to sit out a great deal, and do a good deal of writing outdoors. I have completed most of the chapter on Finger-Prints for the Galton Life in this way. Only in my unfortunate manner, I have been led to try and go beyond Galton, which delays matters! Finger-Prints are very fascinating, and the Laboratory ought, I think, to work more at them. Our usual rule is to sit out in the mornings on a more or less distant bench. At least I sit on the bench, and S—— generally lies on the ground. Then dinner at 12.30! After dinner we go for a rather longer walk, reaching some small *Wirtschaft* or inn about 4 o'clock, where we have coffee. At 7.30 we have supper, a rather more frugal meal than *Mittagsessen*, and then we have read Emil Ludwig's *Bismarck* until bedtime at 10 o'clock. It is strange to find now the grandchildren of the old peasants I knew in 1879 and 1880 in possession of their houses. There are very few even of the intermediate generation left. We have met perhaps half-a-dozen men who were prisoners of war in England. They speak contentedly of their treatment. Those who were not prisoners are apt to speak bitterly of the occupation of the Rhine. Of the more educated classes, it is not easy to find out their views, except in the newspapers. They are very polite, at times friendly, but you cannot find out what they are really thinking....

I am writing this on a bench, where I can just see the faint outline of the Alps. On a clear day after rain we see the whole chain from Mont Blanc to the Tyrolese Alps, but to-day there is a haze, a soft wind, sun, and the peasants coming back from mass.

I can't recall Kepler's features, but we have two or three, I think, authentic portraits in the Laboratory. Mrs Rollo Russell has given us the 'Nature' series framed, which we must find room for somewhere....

I am rather more surprised at your high fraternal than low avuncular relations. You forget that external blood comes into the stirp, when you deal with collaterals?

I am very glad the Index goes forward, but know full well it is a stiff job!

I expect to be back about the 14th.

Yours very sincerely,

KARL PEARSON.

The year 1929 saw the work on the Galton *Life* finished; its publication in 1930 was made possible by gifts from Miss Dorothy Chase Rowell of Columbia University and Mr Henry Mond. So the big task was at last completed and Pearson could feel that he had left a permanent record of the life and work of the founder of Eugenics; with this he was more concerned than with satisfying any immediate public.

"In the centuries to come," he wrote in the *Preface*, "when the principles of Eugenics shall be commonplaces of social conduct and of politics, men, whatever their race, will desire to know all that is knowable about one of the greatest, perhaps the greatest scientist of the nineteenth century. I have endeavoured to put together many things of which the knowledge in another fifty years will have perished, or not improbably the documents on which that knowledge could be based will be distributed in many directions. I have to the extent of my judgment and powers given an account of Galton's scientific work and of his social ideas, so that all that is essential to an appreciation of his labour and thought will be found in these volumes without the need for continual reference to widely scattered papers, and in the future to still more widely scattered letters."

The main volume, III A, contained three chapters, "Correlation and the Application of Statistics to the Problems of Heredity," "Personal Identification and Description" and "Eugenics as a Creed and the Last Decade of Galton's Life;" in volume III B was a long series of interesting Galton family letters and an Index covering the whole *Life*, prepared by Julia Bell. The long third chapter to volume III A, with its many letters exchanged between the founder and the director of the infant Galton Eugenics Laboratory, provides us with a wealth of information, not only about Galton but about those middle years of Pearson's life; we see him from many angles, in friendship, in controversy, in organisation, in scientific research, in relation to his staff, in holiday mood. The reader of the future will hardly regret the introduction of this autobiographical element.

In August 1930 with the Galton *Life* behind him Pearson was at Saig with Margaret V. Pearson, his second wife, who had for many years been a member of the staff of the Department; a short visit was paid to his old university town of Heidelberg on the way back. There remained three years more before he gave up the helm. They were years in which the number of students who came for training in the Department of Applied Statistics was steadily increasing; years in which he published in *Biometrika* some dozen contributions to statistical theory and in which he spent much time and energy over the completion of two other tasks which he had long had before him, the issue of Part II of *Tables*

for Statisticians and Biometricians (102) and of *Tables of the Incomplete Beta-Function* (103).

In the summer of 1933 he resigned his professorship; the decision had been taken in July 1932, so that the College and University might have a full year in which to consider the appointment of a successor and to plan any reconstruction of the Department which might appear to them desirable. The following paragraphs which Pearson had included three years before in his last Report to the Court of the Worshipful Company of Drapers would have served too as a final report to the University of London. What of the future, would those with whom lay the power build or destroy?

"My object during the past forty years has been to build up a Laboratory unique of its kind, a place where a novel calculus should be applied to problems concerning living forms. This purpose involved the development of a new form of mathematical analysis, which has grown largely through the work of my pupils scattered through the world, or through those studying their writings. It will continue to grow, but it will only grow with due sense of proportion, if in touch with practical needs, and if it develops in association with anthropometry, medicine, biometry, and the sciences of heredity and psychology. That is to say, if our new calculus is not to become a field for the exploits of the pure mathematician, it must be linked with investigations into topics where its aid is most needed; it must remain a practical science, i.e. applied statistics.

"I have penned this statement in explanation of the manner in which the Laboratory has been built up and expanded. It is, of course, owing to the smallness of its funds, only the framework of a future structure. But that framework I should like to see firmly established before I leave my post. We need readers in anthropometry, biometry and genetics, especially human genetics.

"Such a Laboratory would have seemed a vain dream forty years ago, but we have gone a long way towards it since then. The most remarkable factor in European scientific progress in this direction has been the development in the last ten years of laboratories precisely on these lines—the combination of anthropometry, medicine, and heredity, with a statistical basis—and this development has occurred in a number of European countries. The Laboratories at Lund, at Berlin and Zürich, are built up on the lines of our work here. But they start with many advantages—beyond a knowledge of our experience; they have ample funds, largely provided by Government or university grants, but also by private donors....

"I am writing these pages fully aware that this may form my last Report on the work done here to the Court of the Drapers' Company, and I do so with the full sense of all that Company has done for thirty years to enable me to carry out the aim of my scientific life, the realisation of my dream of forty years ago; but all that aid, and all the work of building up such a laboratory as the present, will have been in vain if the framework is not maintained, and we leave it to other nations to profit by conceptions originating in our own land. I am much more anxious for the permanent establishment of the Laboratory on a sound footing—the completion of its buildings and the permanence of a highly-trained staff—than for my own few remaining years of office. I admit that the progress of the Laboratory in the future will need careful watching and consideration,

but I would hope that the court would pay in the first place attention to what the Laboratory has done and is designed to do, rather than considering its present director as by long service entitled in any way to a continuance of the grant. If the Laboratory has maintained its reputation for the quality as well as the quantity of its work, it has been in the first place due to the younger generation of trained workers, who have remained faithful to its traditions during the past ten years."

I cannot perhaps do better than close this section with the message Pearson sent through Miss Elderton to his staff and students of that last year on receiving from them an unexpected farewell present of the two large volumes of the Oxford Dictionary:

The Old School House, Coldharbour,
Under Dorking.
September 2nd, 1933.

My dear Dr Elderton,

It was a great surprise to me receiving the Dictionary and your letter this morning. You know through how many years the Laboratory and its Staff have been the greatest of joys to me! Of course it is a very hard task to part with both, but the last two years I had begun to "mind the labour," and felt myself lacking in the requisite energy to cope with obvious difficulties. But the past 22 years will ever be for me the pleasantest of memories, and I hope to keep in touch personally as well as in memory with all old members of the staff. I shall hardly need its delightful present to bear their affections in mind, but they could not have chosen a more serviceable memento or one of greater value to an Editor. Please convey to them all and severally my sense of their goodness and kindness in choice of this keepsake.

Bradshaw once said to me that he held that gifts between real friends should only be flowers—and I added wild flowers. But the world has not reached that standard yet. He once put the fifteen or twenty volumes of Grimm's Dictionary on my table at Cambridge, saying they would be of more use to me than to him; I took it back to his shelves, remarking that dictionaries are not flowers. But I have it now, for I bought it when he died. I will treat your dictionary as the equivalent to a gift of wild flowers in the customary world of to-day.

Always yours sincerely,

Karl Pearson.

Epilogue

The committee appointed by the College decided, after long deliberation, to divide the Department of Applied Statistics into two independent units, a Department of Eugenics with which the Galton Chair would be associated and a Department of Statistics. The existing accommodation, equipment, funds and staff were to be utilised to form these two new Departments. With this scheme Pearson was in serious disagreement. Not only did he feel that the division went against the spirit of all that he had worked for, "a Laboratory, unique of its kind... where a novel calculus should be applied to problems concerning living forms," but he believed that there was almost a breach of trust, since nearly the whole equipment and funds had been supplied in the past for use in such a single

institution. While he agreed wholeheartedly that the College should have a flourishing Department of Statistics which should be primarily devoted to teaching the subject, he could only disapprove of a plan which carved this Department out of the existing research institute, thus limiting the resources available for that establishment. For him, as for Galton, the theory of probability treated from the standpoint of practical statistics was the only sure basis for the study of eugenics, and he feared that this removal of statistics from the Galton Laboratory might result in a future Galton Professor approaching the subject of eugenics from an entirely different basis.

It followed that his retirement from the Department, which must in any case have been a sad event, was made for him more grievous still. He felt that the child of his dreams, this infant laboratory, was to be destroyed by men who had no conception of what it might have grown to. Yet, though the College scheme was approved by the University and a successor appointed to the Galton Chair who was not in sympathy with many of his ideals, though he lived to see his museum broken up and his craniometric laboratory held of small account, with a courage that could triumph over the hardest blows he found joy again in his power still to work. He threw himself into the completion of the monograph on the Cromwell skull and into the editing of *Biometrika*. "When we mind labour, then, then only we're too old."

Was his criticism of the College plan justified and are his fears likely to be fulfilled? That will be for the future to decide. But this can be said now: in the autumn of 1936, about six months after Pearson's death, his old friend Florence Joy Weldon also died, leaving the residue of her estate to found a Chair of Biometry in the University of London, and this has now been established at Uni versity College. The founding of this Chair had been a scheme which she had discussed with her husband over thirty years before, in the early days of biometry. Its association with London rather than with Oxford was no doubt due to the existence in the former place of Pearson's own laboratory. Thus, directly or indirectly, from Pearson's election in 1884 to the Professorship of Applied Mathematics it has followed that three new professorships have since been established at University College, those of Eugenics, of Statistics and of Biometry. It will rest largely with the present holders of these Chairs and their successors whether progress is made towards that goal which Pearson had in view, even if the road to be travelled is not precisely that which he had tried to prepare.

October 1933 saw Pearson established in a room placed at his disposal on the other side of the College by D. M. S. Watson, the Professor of Zoology; it saw also R. A. Fisher as the second Galton Professor of National Eugenics and the present writer as head of the new Department of Statistics. The new order had begun.

In his fresh quarters Pearson had round him his books and the most treasured of his pictures, a close array of great figures from the past and friends of more recent years. Elsewhere in the Zoology building was a store-room for *Biometrika*, where Miss F. N. David, his single research assistant, worked. In the many-sided

labour which the issuing of this journal involved, he received from his wife unstinting help. As long as he was able, he kept as formerly his regular College hours and the College terms; this was the discipline of his science.

The most interesting piece of work which he completed in his retirement was undoubtedly the joint paper with G. M. Morant on the Wilkinson Cromwell Head (116).

"So much has been written about this Head," the authors stated in their introductory section, "and the controversy has been so keen, that it might appear that there was nothing to be said on the topic which had not been said already. In other words that the authenticity of the Head must be ever left in that state of doubt in which historians and critics have enveloped it. Yet when one has studied the innumerable notes, letters, and newspaper articles one finds only a mass of contradictory *opinions*, repetition of various absurd myths about Cromwell's body, not one single trustworthy measurement or fitting of the head to any form of portrait; in fact the whole of the century of discussion is *vox et praeterea nihil*. Had the authors of the present paper merely wished to contribute surmises, criticisms of earlier surmises, vague statements that the Head was in their opinion like or unlike Cromwell's portraits, there would have been no excuse for this monograph. The essential difference between this and earlier discussions of the subject is (*a*) that the authors have no bias for or against the authenticity; (*b*) that they trust solely to measurements on the Head, and to its good or bad fit to portraits; and (*c*) what has been essential to their investigation, that two great privileges have been granted to them by the owner, Canon Horace Wilkinson, (i) to retain the Head adequately long in order to carry on the comparison with busts, masks and portraits, and (ii) to state freely what conclusions they have reached as a result of their investigations."

The investigation was of a kind which appealed to Pearson immensely. "Don't chatter, make trial," Charles II is reported to have said, and though the "trial" which the Merry Monarch may have contemplated was no doubt less exact than Pearson would have approved, the motto served; the more persons who had merely talked about the Cromwell head, the more eager he was to get down to exact measurement himself! The paper followed the lines of the Darnley inquiry, but it covered even more ground. Its authors intended to enjoy themselves thoroughly and anyone who turned over with Pearson the pages of the album in which the first proofs of the 100 illustrative plates were pasted can have had little doubt of the pleasure which the chief author was drawing from his work. The skill and enthusiasm of the historical investigator who, long before, had collected the Veronica portraits of Christ and the woodcuts of Albrecht Dürer were combined with the perfected technique of that old and experienced measurer of heads. And what was the conclusion? Not expressed in terms of a precise probability measure, but approaching that as nearly as possible:

"The defective history of the Head hinders the *demonstration* that it is Cromwell's, but many a man has been hanged on a smaller amount of circumstantial evidence for his crime than exists for the identity in this case. The probability for the identity is so convincing that any critic need not be considered who cannot produce a higher

probability that this Head must be that of another embalmed and decapitated person of the seventeenth century. Who was he, and do his busts or portraits fit to a higher degree this Head?

"We started this inquiry in an agnostic frame of mind, tinged only by scepticism as to whether the positive statements made in the past with regard to it were not based solely on impressions unjustified by any attempt at a scientific investigation. We finish our inquiry with the conclusion that it is a 'moral certainty' drawn from the circumstantial evidence that the Wilkinson Head is the genuine head of Oliver Cromwell, Protector of the Commonwealth."

In the mathematical field also Pearson was still active. Continuing his habit of adding to the supply of readily available tables, he reissued photographically Legendre's *Tables of the Complete and Incomplete Elliptic Integrals* (119), prefacing them by an introduction of his own, explaining the tables and their use. He also put in train the computation of a table he had long planned of the probability integral of the correlation coefficient*. What a part the conception symbolised by that little letter r (which might so easily have been c) had played for forty years in the pages of his statistical contributions!

His paper of 1933 (99) on the new goodness of fit test was followed by another of 1934 containing further applications (120). Finally, we may note two letters to *Nature* (121) and a last contribution to *Biometrika* (122) on a problem which, if the word is understood in its widest sense, may be termed the problem of graduation. Here he sought again to emphasise the difference between the world of concepts and the world of perceptual experience. It is the teaching of *The Grammar of Science*, most clearly seen in the letters, but to be read, too, behind the thrusts of the *Biometrika* article. The mathematical equations of frequency curves and regression lines, the probability distributions of estimates and test criteria, the principles of estimation and of testing hypotheses, all these are abstract concepts the application of which to experience involves a process of graduation. He sensed a danger that statisticians might be carried away by the fascination of ideas into attributing some magic significance to these conceptual models, into giving a false reality to words and phrases whose seeming importance was perhaps enhanced by the addition of capital letters, Efficiency, Power, Information, Likelihood. To him the value of these notions could only lie in their utility in the perceptual world; here there could be no ultimate right or wrong, only more useful and less useful, and even then, what was of greater aid to one man might well be of less to another. Such, I think, was the final statistical message that Pearson left us.

In vacation time he was largely at Coldharbour, where he would still walk his 10 or 12 miles over the hills through Friday Street, Abinger or Holmbury St Mary to the farther end of the ridge. In the last three summers he also returned with his wife to the Yorkshire dales, from which his ancestors "had ridden south over the moor," and spent some weeks at Danby, a place of so many memories. The

* This work has just been completed by Miss F. N. David.

UNIV. OF WICH.

At Saig, with his younger daughter, 1928.

In the study at Hampstead, 1933.

Blakey Ridge, Danby, 1935.

long July evenings of the north, the good smell of turves and bracken and young heather made the worries of London of so much less significance. As he had written to Weldon long before on July 1st, 1900, at the end of a hard College year:

"I breathe pure air again and feel human once more! My forebears were yeomen on these uplands and my great great grandfather was a fool ever to leave them! I should have known all about inheritance and never wanted to give expression to it in a law had he only stuck to the soil! Now I don't possess a square foot of Mother Earth, and can't carry out a single breeding experiment! Just fancy what a flock of 300 sheep would mean!! I have to beg land and labour of friends for a poppy-patch! My Father sold his ancestral patch last year, because we agreed a man ought to farm his own land and it had been let for years to tenants. I had a sort of vague dream of turning it into a breeding farm, but I never quite realised how it was to be kept going. My Father, being one generation nearer the plough than I, considers himself much my superior, but I never heard anything come of his theories or practices in agriculture, while I am certain nothing would come of mine! Still I long to retire to a plot of land and breed something. Pigs or sheep,—poppies or snails are all one to me. Even house-sparrows would be exciting, for I should dearly like to know if egg mottling is hereditary!

"I have written some 35 letters since I arrived here on Friday night,—the arrears of correspondence since Easter, and this is the first letter I don't feel it a nuisance to write and therefore you must pardon my chattering. Since writing so far I have dined and strolled out on to the moor at the back. It is 9 o'clock and broad daylight, and the plover quite active and the grouse whirring off, and nothing when you get over the brow of the hill, but miles of blackish heather, scarcely yet in bud, with the green of the young bilberry shoots, and one gray stone ruin—the bell-house—on the causeway which runs for miles across the moor, which some ancestor of mine built and where he doubtless rung the bell for a guide to the mule-drivers taking wool north and south, when the North Sea 'rauk' came upon them. Then turning to the brow again and looking right up Danby Dale with moor on either ridge and a narrow cultivated strip up the bottom, I see ten miles up the Dale to the Quaker's Way, where in the memory of man, they used to come down riding pillion to the meeting on Sunday, long rides 10 or 15 miles over the moor. Those three little garths like potato patches, each about three miles apart up the dale are really stoneless burial grounds and my forebears lie in them and the natives know neither their name or purpose. The ruin by the middle one more than 200 years ago was Hartus' farm, and George Fox preached there, and my 5th great grandparents were married there, Quaker fashion. Opposite it is Lumley House whence....."

and here the page is lost. Perhaps his eye passed on westwards across the dale to Stormy Hall, with its clump of trees, and Honey Bee Nest, up the track to St Helena, the highest farm of all, and so along the ridge to Blakey, to the hut which had once been a Meeting House on the moor whence in the 1680's the soldiers had taken away to York gaol his ancestors Gregory Pearson and George Unthank, steadfast in their Quaker faith. Or from Lumley House he may have passed to the church in the centre of the dale and up over Danby High-moor to the Fryups, by that track, marked by its tall stones, over which within living memory they had still borne by hand the coffins for burial; and so to the ruins of

Danby Castle, once a manor of the ancestors of the Bruce, and back to the village by the stone causeway where the pack mules had passed and where Tommy Pearson, like Tam o' Shanter, had once joined issue with a witch.

In 1935 it became clear that Pearson's strength was gradually failing; that old hard-worked body was at last worn out though the mind and spirit were still eager to carry on. To the last he worked at *Biometrika*, and he had almost seen the final proofs of the first half of Volume XXVIII through the press when he died. The end came at Coldharbour on April 27th, 1936, when spring in those Surrey hills was at its best.

At the funeral service in London a few days later we heard the words, never perhaps more appropriately spoken, which he had more than once applied to others from his favourite Browning's "A Grammarian's Funeral":

> This man decided not to Live but Know—
> Bury this man there?
> Here—here's his place, where meteors shoot, clouds form,
> Lightnings are loosened,
> Stars come and go! Let joy break with the storm,
> Peace let the dew send!
> Lofty designs must close in like effects:
> Loftily lying,
> Leave him—still loftier than the world suspects,
> Living and dying.

And the music of the second movement of Beethoven's 7th Symphony which followed, told us in its magnificence something of joy breaking with the storm.

In the course of this survey we have seen Pearson from many aspects, as the historian, the writer on folklore, the socialist, the applied mathematician who discussed problems of elasticity and engineering and theories of atomic structure, as the author of *The Grammar of Science*, as the biometrician, statistician and eugenist, as the teacher and the biographer. It would be hard to say which of his contributions to science are of most importance; the influence that he has had does not depend on any sharply defined discoveries. It would be easy, too, to say that here he made mistakes, there his contemporaries or successors in the light of new facts have, in his own terms, found a simpler logical construct than he had, in which to gather the known phenomena of perception. But in such criticism small profit lies. It is well to recall the words he wrote himself on the influence that great minds have had on the generations which followed them:

"The little men say there was evolution before Darwin; the little men say somebody discovered logarithms before Napier, the belittlers believe that the law of the inverse square was propounded before Newton, and that somebody conceived of Eugenics before Galton. Well, the imagination of man has always run riot, but to imagine a thing is not meritorious, unless we demonstrate its reasonableness by the laborious process of studying how it fits experience, or make it a real factor of practice. Darwin did bring the ideas of evolution home to science; logarithms did come into general use after the

publication of Napier's *Logarithmorum canonis descriptio* (1614); Newton did predict the motion of the moon on the basis of his law of gravitation, and the name and idea of a science of Eugenics have become worldwide only since Galton made his appeal and showed its possibilities....

"The little men say that relativity has killed Newtonian mechanics, but they do not add that now and for long years to come satisfactory answers to ninety-nine per cent. of mechanical and physical problems—problems now essential to our daily existence—will be reached by Newtonian approximations....

"Do these statements belittle Einstein? On the contrary the writer believes that while relativity now modifies the treatment of a very small percentage of physical problems, it will in future modify the treatment of more and more. It is a question of the growth in accuracy of our instruments and the developing refinement of our observational powers. The fundamental importance of relativity at the present time is the manner in which it is changing and must change our attitude towards the physical universe....New phases of philosophy, new phases of religion will grow up to replace the old. But the cultivated mind can never regard life and its environment in the same way as men did before those days of Darwin and before these days of Einstein. The 'value' of words, the 'atmosphere' of our conceptual notions of phenomena, has been for ever changed by the movement which began with Darwin and at present culminates in Einstein." ((117) pp. 5—7.)

Can we not perhaps say that in similar manner, by the long process of studying how it fits experience, Pearson has made the calculus of mathematical statistics a real factor of practice in vast fields of scientific inquiry? Not only did he display the motto, "Until the phenomena of any branch of knowledge have been submitted to measurement and number it cannot assume the status and dignity of a science," but having himself provided a mathematical technique and a system of auxiliary tables, by ceaseless illustration in all manner of problems he at last convinced his contemporaries that the employment of this novel calculus was a practical proposition. From this has resulted a permanent change which will last, whatever formulae, whatever details of method, whatever new conceptions of probability may be employed by coming generations in the future. And if in Pearson's work a critical eye can find here and there a blunder in algebra or arithmetic, or an apparent lack of clearness in thought, it will do no harm to remember his own statement in *The Ethic of Freethought*:

"Every freethinker, then, owes an intense debt of gratitude to the past; he is necessarily full of reverence for the men who have preceded him; their struggles, their failures and their successes, taken as a whole, have given him the great mass of his knowledge. Hence it is that he feels sympathy even with the very failures, the false steps of the men of the past. He never forgets what he owes to every stage of past mental development."

In the spirit of these words let us leave him, paying reverence to a great man who has preceded us, and confident that wherever the path of science may lead, Karl Pearson has contributed his full share of that pioneer work from which alone true progress can follow.

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APPENDIX III

EXTRACT FROM KARL PEARSON'S REPORT TO THE WORSHIPFUL COMPANY OF DRAPERS MADE IN FEBRUARY 1918

War Work of the Biometric Laboratory

Since the Report of 1913 made to the Court of the Worshipful Company of Drapers on the work undertaken by aid of their grant, the war has given a wholly different course to the life of the Laboratory and its staff. In July, 1914, we fully expected the main work of the next six months to be the occupation and equipment of the new Laboratory buildings, the fitting up of the public museum and the anthropometric laboratory. All this development and the extension of the biometric work which would have followed it were shattered by the war. The new Laboratory buildings were taken

over by the Government as a military hospital and will presumably be used as such till the end of the war. Very early in the war several members of the staff went off on special war duties for which their training in computing largely fitted them. Of those who in July, 1914, were at work in the Laboratory, Mr Soper left to do experimental work on electrical apparatus for war purposes; Mr Everitt left to train women in the polishing of prisms and lenses for periscopes, etc.; Miss B. M. Cave went as a computer to the Admiralty for naval air-plane work; Dr Heron left as statistical adviser to a large insurance company, of which he has since become secretary; he has further acted as a statistical adviser to the Ministry of National Service. Amongst those who filled the gaps thus arising, Mr Horwitz has since gone as a statistician to the Ministry of Munitions, Mr Firth in a like capacity to the Contracts Department, War Office, and he has recently been followed by Mr Frobisher, the Crewdson-Benington Student, who felt it his duty to ask for the suspension of his studentship that he might undertake similar work under the War Office. In all these cases age or physique disqualified for active military service.

These matters are cited as illustrations of the difficulty at the present time of holding together for pure research work a highly trained staff. Posts could have been found in Government offices at the present time for double the number of my total staff, and many old students and past assistants of the Department are at present employed in one form or another of statistical work, often of a very important or confidential character, for the War Office or the various new Ministries.

The position therefore was at the beginning of the war an extremely difficult one. It was essential for the future to retain if possible a highly trained staff, but the funds at our disposal neither enabled us to compete with the high salaries offered to competent statisticians, nor, if they had been, would it have appeared justifiable to keep members of the staff, who were urgently needed for national work of importance. The only reasonable solution of the difficulty seemed to be the voluntary employment of the Laboratory as a whole on war work, and this in some form wherein its training and computing experience would be of essential value at the present crisis. The feeling that the staff as a whole were doing national work would maintain its *esprit de corps* and retain its more loyal members at their posts, even if more highly paid appointments were proposed to them. Accordingly I discussed the matter with the staff in the first week of August, 1914, and its members agreed to dispense with the best part of their holidays, and to devote their time to war work. With hardly an exception this attitude has been maintained throughout the whole period of the war up to date by my old staff. They have worked to the full extent of their powers and sometimes beyond them, holidays have been few and far between and only taken when some rest was a necessity. I cannot speak too highly of the loyalty and energy of my assistants. In 1916 the Laboratory was kept going throughout the whole of Easter, and for the men the hours have been 9 to 6, Sundays only excepted.

In August, 1914, we started with work for the Board of Trade, Labour Department, the question of unemployment being then a vital one. We prepared fortnightly labour charts, showing the state of unemployment both for insured labour and uninsured labour in all English, Irish and Scottish towns of over 20,000 inhabitants, and in all county districts. Some 600 charts were prepared for each issue and brought up to date. These

were used by the Central Relief Committee for the control of local conditions. By July, 1915, our charts themselves showed that the possibility of great labour difficulties which had been so marked in December, 1914, had vanished, and accordingly these charts ceased. We next worked for the Board of Trade, Census of Production Department, preparing charts of the tonnage used for each type of import at each season of the year with a view to aiding the special officials who had the task of controlling the amount and character of imports and the shipping to be used for them. These charts were followed by a series of charts of the rates of exchange in all the great European and North and South American cities. These were photographically reproduced and kept up to date for a whole year, being distributed by the Board of Trade to various Government offices. Meanwhile more urgent problems had come to us from other sources. During a good deal of 1916 we were occupied with theory and computations concerning torsional strain in the blades of air-plane propellers for a department of the Royal Aircraft Factory at Farnborough. A report was also prepared on the elastic constants of wood for the head of a section of the Admiralty Air Department. In July, 1916, our energies were directed by a member of the same department to bomb trajectories, and several months were devoted to the calculation of such trajectories for use in the sights of bombing air-planes. Our tables have been privately printed by the Air Committee. This work was later extended to combined air and water bomb trajectories in which a number of new problems arise, and we endeavoured to consider them experimentally by using models. The successful solution of these problems would be of great importance in the anti-submarine campaign.

Thus our work during 1916 gradually turned to the more military side of war work. On January 1st, 1917, we were asked to assist Captain A. V. Hill, of the Anti-Aircraft Experimental Section of H.M.S. Excellent, with gunnery computations for anti-aircraft guns, and from that date onwards we have been engaged without cessation in computing ballistic charts and range tables for the Ordnance Committee. We have had in charge the preparation of the whole of the charts and high-angled range tables for the anti-aircraft guns in both Army and Navy, and the preparation of the data for the sights of these guns. All the organisation and control of the work, all the finer draughtsmanship of the charts, was undertaken by the trained members of my staff. The charts have been engraved by the Ordnance Survey and now number twenty. The high-angled range tables are printed at Woolwich, and both charts and range tables are now issued officially by the Ordnance Committee for about a dozen anti-aircraft guns. The work has been so urgent and of such value that the Ministry of Munitions has placed eight to ten computers and draughtsmen at my disposal, and with the exception of one week at Christmas the Laboratory was never closed from January 1st, 1917, to January 1st, 1918.

The main feature of the work, however, has been the voluntary work of direction and control exercised by my staff. Only recently, owing to the rise in prices and the small stipends paid to academic workers, have *honoraria* for holidays and overtime been paid to the junior members of the staff. Voluntary enthusiasm has been the mainspring of the whole enterprise. I venture to think that we may claim it as good evidence of the value of the training given in the Laboratory that our members could thus take upon themselves an entirely new field of work. It must be remembered that high-angled range tables for anti-aircraft guns were unknown before the war had developed the

aeroplane as a new instrument of warfare, that in the great bulk of cases we had to develop new methods from the very rough processes originally suggested to us, and that the authorities at Woolwich have themselves consulted us as to methods of calculation and instruments used. In the course of the work no fewer than 85 new tables have been computed for various guns, giving ranges, fuzes or sights, and, further, several of the old ballistic tables have been re-calculated or developed to higher degrees of accuracy. We have heard from men at the Front due appreciation of our charts and tables, and whereas before their existence practically no air-planes were shot down, we now hear of upwards of sixty in six months by direct anti-aircraft gunfire.

Just before Christmas an urgent demand came from the Front to General Bingham for a remedy against the low-flying German air-planes, which were making things "unhealthy" for our men in the trenches. It was a source of great satisfaction to us, and a recognition of the work done, that we were at once asked by the Ministry to undertake the urgent work of calculating sights for the Hotchkiss, Lewis and Vickers machine guns to meet the cases of air-planes flying with various speeds in various directions and at various low altitudes. The task was a novel and difficult one, but the theory was worked out in the Laboratory, and within four or five weeks of the order the tables were sent to France, arriving there just before Christmas. At present we are occupied with the wind influence on firing at high altitudes, and with new tables for naval high-angled guns, owing to the adoption of a new fuze by the Admiralty.

Samples of the war work of the Laboratory are enclosed in a portfolio accompanying this Report. It must of course be remembered that they are of a confidential character. They are evidence at any rate of the activity of the staff in my charge. I venture to think it would come as a grave blow to these young people to hear that at the present time the Court had not found it possible to maintain the grant. We have given of our best where it seemed from the national standpoint to be most urgently needed at the present moment, and it has meant work of a most strenuous and long-maintained character.

My object throughout has been to maintain a body of trained computers together who would have the force of character and the knowledge to meet new problems and remain as a nucleus for the Laboratory research work when peace returns.

(Signed) KARL PEARSON.

A copy of the following letter from Vice-Admiral R. H. Bacon accompanied the Report:

MINISTRY OF MUNITIONS

Princes Street, Westminster, S.W. 1
14*th February*, 1918

Dear Professor Pearson,

Captain Moore has brought to my notice the letter which you addressed to him on the 13th February, and is, I understand, returning the communication from Major Douglas which accompanied it. I wish, however, to take this opportunity of expressing my cordial appreciation of the very valuable assistance which the laboratories under your charge have rendered to the Ministry in general, and to this Department in particular, and thereby to the Country during the War.

At a time of great pressure the Ministry found itself in need of a trained staff of computers provided with necessary machines to undertake gunnery work, and was indeed fortunate to find such a staff and machinery already in existence at the Drapers' Company Biometric Laboratory and the allied Galton Laboratory. The work done in connexion, *inter alia*, with the preparation of practically all the charts and high angle range tables for the a.a. guns of both Services has proved of inestimable value; and it was no small advantage to find at a time of national stress that a school had been trained in times of peace for such computing work as became, on the outbreak of war, a matter of such vital importance.

I leave it of course to your discretion to make what use you may please of this letter, but I think it would be very fitting if you were to bring to the notice of the Court of the Drapers' Company the very great value attached by the Ministry to the services which have been rendered by a laboratory which, I understand, owes much to the traditional generosity and public spirit of one of the great city companies.

Yours faithfully,
(Signed) R. H. BACON
Vice-Admiral, and Controller, Munitions Inventions.

APPENDIX IV

SUMMARY OF SUBJECTS DEALT WITH BY KARL PEARSON IN HIS TWO LECTURE COURSES ON THE THEORY OF STATISTICS GIVEN AT UNIVERSITY COLLEGE, LONDON, DURING THE SESSION 1921–1922. The material is taken from E.S.P.'s lecture notes of that date. (See pp. 208—211 above.)

FIRST YEAR COURSE, 1921–1922

First Term

Lecture*

1. Introductory outline; the conception of correlation as distinct from causation. Classification into qualitative categories, the contingency table and conception of independence. Characters on a quantitative scale, x and y; means of arrays, the correlation ratio; special case of the regression straight line; the coefficient of correlation.

5. Polynomial regression lines; the least square principle leading to the equating of moments. Definition and properties of moments; Sheppard's Corrections. Return to fitting polynomial regression lines; orthogonal functions. The study of variation about the regression line in an array leading to the study of frequency distributions.

9. Discrete variates: the binomial type of problem, cause groups at each trial independent; the hypergeometric type, cause groups not independent. Detailed study of the binomial; need for approximation leading to use of its moments. Attempt to graduate the binomial from the ratio of slope to ordinate leading to a differential equation. Solution of this equation gives (i) in special case the Normal curve, (ii) in general case, the Type III curve. Properties of the Type III curve.

* I have not indicated the point at which each individual lecture started but the figures in this column will show roughly how many lectures were given to different parts of the subject.

Lecture

14. Properties of the Normal curve. Representation of data on a normal scale. Gauss' work regarding the arithmetic mean, the mean square deviation and the method of least squares.

16. The Poisson limit to the binomial, its moments and uses.

17. Introduction of the more general differential equation whose solution gives Pearson's system of frequency curves. The symmetrical curves, Types II and VII. Types XII, V and IV. [End of term after 22nd lecture.]

Second Term

23. Completion of Type IV curve. Types I and VI. Special curves, VIII, IX, X and XI.

30. Recapitulation of work on frequency curves.

32. Corrections for grouping to be applied to moments. The Euler-Maclaurin Theorem. Abruptness corrections.

36. The fundamental problem of Statistics—to predict from the past what will happen in the future. Bayes' theorem, criticisms of Boole, Venn and others; the "equal distribution of ignorance." Suggested extension of this theorem. Work of Laplace; the Normal curve derived as an approximation to a hypergeometric series; the Type I curve as a much better approximation to this series.

42. "Probable error" theory. The sampling variation and co-variation in group frequencies. Large sample as distinct from small sample theory; original population assumed very large compared to sample; nature of approximation involved in substituting sample for unknown population values. The standard errors of moments calculated about a fixed origin, e.g. of the mean. [End of term after 43rd lecture.]

Third Term

44. Approximation to the standard error of a standard deviation. Use of R. A. Fisher's 3 and then n-dimensional space transformation to obtain sampling distributions of mean and standard deviation for a Normal population. Properties of the standard deviation distribution. Sampling moments of the squared standard deviation for any population.

48. Derivation of first-order approximations to the sampling moments and cross moments of moments. The standard errors of β_1 and β_2 to a first approximation; application of these results in choice of Pearson frequency type from observed β_1 and β_2.

51. The relation between variation in the means, standard deviations and frequencies of arrays in samples from a bivariate distribution; a test of significance for the squared correlation ratio.

55. The correction to be applied to the mean square contingency, ϕ^2, in a two-way table. [End of Session after 55th lecture.]

SECOND YEAR COURSE, 1921–1922

First Term

Lecture

1. Indices; approximations to their standard errors; spurious correlation.

2. Multiple correlation and regression; general problem: to predict x_0 from knowledge of $x_1, x_2, \ldots, x_n$. Regression function, $X_0 = \phi(x_1, \ldots, x_n)$; investigation confined to case where ϕ is a linear function of the x's. Constants in linear equation obtained by maximising correlation between x_0 and X_0; introduction of determinants in solution. Relation between n and possible magnitude of correlations of type r_{os} and r_{st}. Application to prediction of characters in offspring from knowledge of characters in ancestors; the effect of environmental as compared with hereditary factors. Partial correlation.

6. Frequency surfaces for 2 or more variables; consequences of assumption that these depend upon a large number of underlying independent factors. The multivariate Normal surface. Special study of properties in the bivariate case; transformation to 2 independent variables; Sheppard's formula for calculating correlation.

10. The 4-fold table and tetrachoric coefficient of correlation; properties of tetrachoric functions.

13. The transformation to be applied to the mean square contingency, ϕ^2, to obtain an estimate of r, i.e. $C_2 = \sqrt{\phi^2/(1+\phi^2)}$. Class index corrections for broad grouping. Biserial and triserial r and η.

16. Standard errors of frequency constants in samples from bivariate distributions. $p_{uv} = \Sigma(n_{xy}x^u y^v)/N$. *Case* (*a*). x and y measured from fixed origin. Expectation of $(\delta p_{uv}\delta p_{u'v'})$; statistical differentials and mathematical differentials. Approximations used justified in case of large samples; no assumption of normality. Application to problems of selection. Illustration in obtaining the standard error of r. Reference to R. A. Fisher's distribution of r and its great variety of frequency forms in case where population is Normal.

20. *Case* (*b*). Expectation of $(\delta p_{uv}\delta p_{u'v'})$ when x and y are measured from sample mean values; problems of selection again referred to; illustrations: $r_{\sigma_x \cdot \sigma_y}$, $r_{r \cdot \sigma_x}$, $r_{r \cdot \bar{x}}$. [End of term after 21st lecture.]

Second Term

22. The Variate Difference Method of investigating correlation; discussion of some of Pearson's work in progress; effect of periodic terms.

28. Further applications of multiple correlation; the χ^2 test derived from consideration of correlated deviations in group frequencies. Problems associated with χ^2; the effect of substituting a fitted curve for population curve in goodness of fit tests.

31. Multiple regression formulae applied to inheritance; the Law of Ancestral Heredity, a *mass* law. Galton's work. General hypothesis of the Mendelian Theory; correlations to be expected to arise between relatives in a population mating at random. Form of the Ancestral Law to be expected on Mendelian theory.

Lecture

37. The standard error of the median. The standard error of the estimate of the standard deviation obtained from the quartiles; reliability of estimates of standard deviation calculated from other percentile points (all for a Normal population). Outline of method to be followed in determining the standard error of a "class index correction." [End of term after 39th lecture.]

Third Term

40. Further suggestions on the "class index correction" problem.

41. The method of Correlation of Ranks.

43. The standard error of the tetrachoric coefficient of correlation.

45. The analysis of compound material; the breaking up of a frequency distribution into two component Normal distributions.

48. Galton's Individual Difference Problem: the expected value of the difference between the pth and $(p+1)$th individuals in order of ranking, in a sample from a Normal population.

50. A further application of multiple correlation theory: the influence of selection applied to one or more characters on means, standard deviations and correlation coefficients. [End of Session after 50th lecture.]

BRIGHT'S DISEASE, NEPHRITIS AND ARTERIO-SCLEROSIS: A CONTRIBUTION TO THE HISTORY OF MEDICAL STATISTICS

By MAJOR GREENWOOD, F.R.S. and W. T. RUSSELL

From the London School of Hygiene and Tropical Medicine

Introduction

In the course of our ordinary academic and official work, we make daily, almost hourly, use of the various reports of the General Register Office. Increases or decreases of the rates of mortality from all causes and from particular causes naturally attract our attention. The vicissitudes of these rates arouse our curiosity, we wish to know how the changes recorded arise. We know that these arithmetical statements are the tabulations of opinions expressed by medical practitioners and that changes in them will be determined by three principal factors. The first is a change in the frequency of a cause of death the nature of which has always—which means, of course, for statistical purposes, during the last hundred years—been recognized by certifying practitioners and described by them in the same terms. The second is a change of opinion leading practitioners in one age to prefer cause *A* to cause *B*, but in another generation to prefer cause *B*, or perhaps to choose a cause *C* which their fathers had not recognized. The third, correlated with the second, is change in the grouping of causes, too numerous for separate tabulation, adopted by the central statistical department. All specific rates of mortality are influenced by the first and second factors, many by the third. The third factor cannot always be measured but can be ascertained by a diligent study of the official documents themselves. The second, on the other hand, usually presents an intellectual problem very difficult to solve. Nobody who is even slightly concerned with medical statistics is ignorant of the controversy regarding the real measure of increase of mortality from cancer. Almost all would agree that the decline of the rate of mortality from tuberculosis is one of the most certain of statistical results. Yet even in this matter, a reader of such a critical study as Rosenfeld's *Tuberkulosestatistik* (League of Nations, 1925, C.H. 284) must agree that there is room for doubt as to the extent of the change.

From the purely arithmetical aspect, mortality attributed to nephritis is a striking example of vicissitudes. Beginning as a statistically negligible factor it rose to be one of the arithmetically important causes of death, and rose steadily for decades. Then, shortly before the war, it began to decline and, although it

has never shown signs of reverting to its original statistical insignificance, is of much less importance than 25 years ago.

Although one of us has had no clinical training and neither of us any clinical experience more recent than 30 years ago, we were not ignorant of the fact that the work of the last century had greatly changed medical opinion respecting the importance of disease of the kidneys. It seemed clear enough that these changes of opinion must have affected the statistics greatly. On the other hand we knew vaguely that some forms of disease of the kidney had been attributed to intoxications which might arise in connexion with industrial processes not in use 60 years ago and we knew quite distinctly that one form of nephritis, that arising in connexion with scarlet fever, was much less frequent than a generation ago. Hence we could not be sure that the statistical changes were wholly changes of opinion. It seemed to us, therefore, that a general examination of nephritis as a statistical cause of death would be interesting. We were sensible of our lack of equipment for making this study, but we hoped that an imperfect attempt might stimulate better trained persons to enter on this field of research. Nephritis is only one of a number of problems awaiting examination.

Knowledge of nephritis

The ultimate data of the medical statistician are the opinions of medical practitioners and he is not, strictly speaking, concerned with the question whether those opinions are correct. So, a cynic might argue, we are not concerned with the scientific truth—even if we knew what it was—but with what ordinary men believed to be the truth. But, as the conclusions reached by scientific investigators do, eventually, become common property, it will be interesting before one concentrates upon common opinion to try to give a very brief account of the best opinion of successive generations.

The pre-Galenical physicians drew many and correct inferences from the character of a patient's urine, but they did not have, or are not known to have had, any clear ideas on the function of the kidneys. Galen was, of course, an accomplished physiologist. His account of the rôle of the kidneys in *de Usu Partium* (see Kühn's ed., vol. III, pp. 273, 362 *et seq.*) is clear and he had no doubt at all that the function of the kidneys was to separate waste matters from the whole of the blood; that was why, in his opinion, their veins and arteries were so large, and he mocked at those who held the urine to be a mere product of local metabolism; indeed his remarks on the reason for the dense texture of the kidneys might be taken for a foreshadowing of a filtration hypothesis. He had some knowledge of renal pathology and defined nephritis as a phlegmon of the kidneys accompanied with pain (Kühn, XIX, 424). He had an inkling of the relation between some forms of dropsy and visceral fibrosis and defined the vermicular pulse of patients with dropsy having this origin (Kühn, IX, 312). But Galen did not systematically co-ordinate his physiology,

pathology and clinical observations. In the thirteenth century William of Saliceto (see R. H. Major's *Classic Descriptions of Disease*, p. 483; Thomas, Springfield and Baltimore, 1932) recognized the association of dropsy with scanty urine in life; *durities in renibus*. Ettmüller in the sixteenth century also recognized the connexion. But in the tractate on dropsy by the illustrious Sydenham there is no hint that he had even so clear an idea of the function of the excretory system as Galen. Dropsy he attributed to weakness (*debilitas*) of the blood. Sydenham's contemporaries Morton and Cole had more insight into pathology. But Morton's remarks on dropsy in phthisis deal with hepatic not renal functions (Morton, *Opera Medica*, ed. 1697, p. 131). It is a pity that Cole did not include renal functions in his general treatise *De secretione animali*. There are only cursory references (pp. 135, 157).*

Sydenham was a great practitioner; he remarked in this tractate that just as Hippocrates blamed the officiousness of those who preferred speculation to practical observation, "so may a prudent physician of the present time blame those who believe that medicine is to be promoted by the new chemical inventions of our day, more than by any other process whatsoever" (sect. 23). He would, no doubt, have attached little importance to his contemporary Dekkers' (1648–1720) observation that in some urines a drop of acetic acid produces a white coagulum (Major, *op. cit.* p. 485) and not much to that of Cotugno (1736–1820) that the urine of a soldier with dropsy coagulated on heating. Cotugno's work appeared in 1765 (see Dock, *Annals of Medical History* (1922), IV, p. 287); nearly 60 years passed before anything was added. Then within two years of each other a paper by W. C. Wells and a treatise by John Blackall (abstracts in Major, *op. cit.* pp. 487–93) made Cotugno's point firmly. Both writers quoted several instances of dropsy with heat-coagulable urine, both referred to associated changes of the kidney of a sclerotic form in some cases followed to autopsy. Wells was an eminent London physician, Blackall a leading practitioner in Exeter. Richard Bright did not begin practice in London until three years after Wells' death but might have met him. There can be little doubt that these writings influenced Bright, who, so far as general opinion is concerned, is the pioneer of the subject.

Down to this year, Bright's, literally, epoch-making papers had not been reprinted in English, although a good abstract of the first and complete translation of the second formed volume XXV of *Klassiker der Medizin*, edited by the late Karl Sudhoff. Now an excellent edition, edited by Dr A. Arnold Osman, is available (*Original Papers of Richard Bright on Renal Disease*; Oxford Medical Publications, 1937).

Perhaps no single contribution to medical knowledge has received such early and general approbation as that of Bright. In Mackintosh's textbook (*Principles of Pathology and Practice of Physic*, by John Mackintosh, 4th ed., London, 1836)

* Printed in the edition of Morton's works above quoted.

Bright's first paper is abstracted at length, and for more than a generation the textbook accounts were simply abstracts of what Bright had said.

A reason for this rapid acceptance of Bright's teaching was its clarity and congruence with English habits of mind. In the first report (1827) Bright did not beat about the bush or indulge in speculations, but went straight to his mark. He recognized that many morbid conditions might be clinically associated with dropsy, but that when dropsy was associated with the excretion of albumen in the urine he had always found the kidneys to be diseased. He did not even claim that the disease of the kidney was necessarily the primary cause, but suggested that the altered excretory function might result from many factors either destroying the balance of the circulation or causing direct inflammatory changes in the kidney. It is material to note that of the 23 cases minutely described in this paper, many were of *acute* disease in young persons, indeed six of the patients were under 30 years and two under 20. In 9 of the 23 cases, there was a history of alcoholic intemperance.

Bright's next communication was in the Goulstonian lectures of 1833 (reprinted by Osman, pp. 153–66). Here again he recognizes the possibility of the renal disease being a secondary effect, but remarks (pp. 165–6) that he has observed the gradual approach and increase of cardiac hypertrophy coming on months after the albuminous condition of the urine had been established and suggests that cerebral symptoms may be due to the cardiac derangement, itself secondary to kidney disease.

Bright's report of 1836 is his fullest account and should be read carefully—by no means an irksome task for it is an admirable piece of writing. The title is significant: "Cases and Observations, illustrative of Renal Disease *accompanied with the Secretion of Albuminous Urine*." The italics are, of course, ours. The point is that this criterion must increase the proportion of acute cases included in any sample of cases.

It would probably be correct to say that Bright regarded the passage of albumen into the urine as an essential element of the diagnosis, although he definitely says (pp. 96–7) that albumen may not always be found and is led to make some remarks (p. 98) which, had he been a Greek author or even Sydenham, would certainly have been claimed as an anticipation of views expressed after his time. Whether Bright would have regarded *any* morbid condition of the kidney associated with albuminuria as within his group is not certain. But, as Osman shows (p. 168), one of Bright's typical cases was of amyloid disease. We shall now briefly analyse the report.

Bright begins by expressing the opinion that this disease is amongst "the most frequent, as well as the most certain causes of death in some classes of the community, while it is of common occurrence in all; and I believe I speak within bounds, when I state, that not less than 500 die of it annually in London alone".

He gives next a vivid account of a typical history. At the outset we are conscious of a difficulty. The opening words of his typical history are these: "A child or an adult, is affected with scarlatina, or some other acute disease; or has indulged in the intemperate use of ardent spirits for a series of months or years." He then passes to the clinical description. A few sentences earlier he had spoken of intemperance having laid a foundation and remarked that "a more impressive warning against the intemperate use of ardent spirits cannot be derived from any other form of disease with which we are acquainted; since, most assuredly, by no other do so many individuals fall victims to this vice" (p. 94). But of the 10 cases described in detail the personal particulars are these. No. 1: The patient was a physician of 42 "who had always lived freely but not intemperately". He was first seen in 1832 and had had symptoms of albuminuria and dropsy for two years. He died of apoplexy in 1834. No. 2: The patient here was also a medical practitioner, aged 33, and he had had scarlet fever 9 years before Bright examined him. His symptoms went back several years and he died with signs of what we should call uraemia about 4 months after Bright had first seen him. No. 3: A youth of 17, who died in uraemia; he had had haematuria with calculus at 12. The fourth case was not of one of Bright's patients and the personal particulars are scanty. The fifth case was of a man of 25 who also died in convulsions and coma. He presented himself complaining of dyspepsia and dimness of sight. The sixth patient, a woman of 24 who died with convulsions and in an apoplexy, had a history of albuminous urine over 4 or 5 years. The seventh case, again of a young woman of 21, had a 15 months' history of dropsy. The eighth case was of an Irishman of 50 who died "with decided cerebral symptoms". The ninth of a marine aged 43 who had lived a very intemperate life. He died from peritonitis. The tenth was of a youth of 21 who also died from peritonitis.

Of these 10 patients, at least 6 were under 35 and one only was *certainly* an habitual spirit drinker. One case was evidently post-scarlatinal. In this particular sample, then, the aetiological importance of alcohol must have been slight. The 100 cases followed to autopsy recorded at the end of the memoir have few personal particulars except age which is recorded in 72.* These have the following age distribution:

– 5	0
–15	2
–25	7
–35	18
–45	16
–55	22
–65	6
–75	1
	72

* Bright (p. 151) says 74, but we can only find 72; probably a couple of figures were slipped in printing off.

So 37·5 per cent. of these were under 35, a much smaller proportion than in the sample of ten, but a considerable proportion.

We have already seen that the explanation of this is the criterion of selection, viz. by an albuminuria demonstrable with the means available a century ago. But, in his discussion, as distinct from his clinical records, Bright plainly attached much importance to alcoholism; he probably thought the non-acute cases of greater numerical importance than they had in his data. Thus he stressed the insidious nature of the disease and carried out some statistical experiments suggestive of Louis' methods. He had the urines of 130 patients in the wards of Guy's Hospital in the winter of 1828–9 tested for albumens. Of 130 tested, 18 had urine coagulable by heat and in 12 others traces of albumen were found. He then showed on a sample that the patients with coagulable urine had indeed disease of the kidney. Friends and pupils at Guy's repeated the experiment on 300 and 141 patients, and Bright concluded that "the disease, in its various stages, from its earliest functional derangement to the confirmed organic malady, is one of the most frequent, as well as of most fatal occurrence: and I think I am fully borne out in the estimate, which I made at the commencement of this paper, that not less than 500 deaths annually occur in this metropolis, from this single disease" (p. 122). We should not be statisticians if we were not curious to learn how that particular figure was obtained. Bright does not satisfy our curiosity. However, those who like figures may be amused by the following.

In the year 1913, almost the *statistical* high-water mark of Bright's disease, 2009 deaths in London were classified under that heading. The population of the Administrative County of London in 1913 was perhaps two to three times that of Bright's London, so, *judged by this criterion*, his estimate was not excessive. In the memoir of 1836 Bright does not add materially to the pathological descriptions given and beautifully illustrated in his 1827 memoir. There he had reduced the macroscopic types to three. It is beyond our province to discuss these but it is fair to quote Bright's own words. "Although I hazard a conjecture as to the existence of these three different forms of disease, I am by no means confident of the correctness of this view. On the contrary it may be that the first form of *degeneracy* to which I refer never goes much beyond the first stage; and that all the other cases, including Sallaway, together with the second series, and the third, are to be considered only as modifications, and more or less advanced states of one and the same disease" (p. 70). Sallaway's case, as mentioned above, was one of amyloid disease.

Perhaps our account has been sufficiently detailed to justify the statement made earlier, that Bright's teaching was well calculated to produce an impression on the medical world. He had linked up such common afflictions as dropsy and such frequent modes of death as apoplexy and convulsions with a disease of the kidneys demonstrable by a simple test during life and upon a

plate after death. He had carefully refrained from exaggeration. He did *not* say, or even hint, that all cases of dropsy or all deaths from apoplexy or in convulsions were due to renal disease. He did not even suggest that an examination of the urine was an infallible diagnostic test. He *did* direct the attention of clinicians and pathologists to a new world of ideas. If any work deserves the epithet classical, his did.

Dieulafoy remarked many years ago but long after Bright's time that a signal merit of Bright was his caution, his abstention from dogmatizing about the aetiology of the morbid processes he described.

THE PATHOLOGY OF NEPHRITIS

To sketch the history of medical opinion even down to Bright's day was a little presumptuous in two statisticians whose combined stock of clinical experience is confined to what one of them learned as a medical student and assistant to a general practitioner 32 years ago. To go further is the height of rashness. But, if this memoir is to be of any use to the statistical reader, some attempt must be made to explain the difference between the point of view of a physician of Bright's professional standing now and that of Bright. In trying to give this explanation we shall probably fall into two snares; on the one hand we shall miss the significance of some points owing to technical ignorance; on the other something of what we say will be unintelligible to a statistician who has not read an elementary book on physiology. That is the inevitable fate of workers in borderland subjects. We ask for forgiveness.

Although Bright was a contemporary of the founders of modern pathology and physiology, the methods of research they began to use were not then instruments of precision. Bright's pathological anatomy was naked-eye anatomy; he could not have minutely investigated the detail of the changes in the kidney, or other organs, by means of serial sections and differential methods of staining the tissues. He had no means of exactly testing the biochemical efficiency of the kidney as an organ of excretion. The results of experimental interference with the kidney in other animals known to him were few. Even the technique of co-ordinating clinical records was in its infancy. He had to depend almost wholly upon his own observations.

The position Bright had reached was this. He had shown that a diseased state of the kidney might arise in a number of ways. First of all some poison, for instance that of scarlet fever, might excite an acute inflammatory condition of the essential elements of the kidney structure. The patient might be killed almost at once by this acute disease. That was one important set of cases. Then the patient to whom this accident had happened might survive, but survive maimed. Perhaps years afterwards, perhaps as the result of another accident, say some other acute illness, the *locus minoris resistentiae* gave way, and death resulted. That was another group, the acute became chronic and then either by

sudden failure or by slow deterioration, death resulted. Lastly, he had patients who had not sustained a pathological accident but through fault of constitution or fault of living (the alcoholic habit, perhaps) damaged their kidneys and eventually broke down with mortal symptoms and signs which could be debited to the renal function. Here there was never an acute stage of inflammatory disease at all. To use a favourite medical term, the onset was insidious. This group, not by any means a majority in his *statistical* experience, but important in his experience as a physician, has probably or certainly been the most important element in the official statistical record of Bright's disease and changes in knowledge of the aetiology of this kind of disease a principal factor of statistically recorded changes.

Progress since Bright's time has been of two kinds. On the one hand there has been what one might almost call a deductive process. Starting with the postulate that the kidney is diseased—we leave for a moment the question *how* diseased—one may set this problem. Why and how does the circulatory system become involved? Under what circumstances will dropsy, oedema, be produced? One has a problem in bio-hydrostatics, to the solution of which most of the great physiologists and pathologists of the last century have contributed. *Pari passu* one has an inductive or descriptive advance. An autopsy becomes not the examination of an hour or two but, in the aggregate, of months or years. The exhibits, as the writer of a detective story would say, in any one case give the micro-anatomy not of one but of many organs. Material is accumulated by means of which the micropathologist can deduce the chronological order of the morbid changes as between organ and organ and between one and another part of the same organ. Co-ordinated with this, one has an improving system of clinical records and therefore the means of correlating the stadia of anatomical change with symptomatic change.

Quite early in the history of advance, indeed less than 20 years after Bright's death, some pathologists had reached the conclusion that a proportion of the cases with, to use another favourite phrase, the clinical syndrome of Bright, took origin not in a primary lesion of the kidney but in a change in the kidney secondary to a degenerative process in the smaller arteries. In English textbooks, Gull and Sutton (1872) have the credit of pioneers. Whether justly or not, we have not inquired. At least out of such work developed the conception of arterio-sclerosis, which, as we shall see, became statistically of great importance in the early years of the century.

We do not propose to try to tell the story of the 60 years' progress. The two memoirs by Russell (1929) and Gray (1933)* will give any reader a view of the methods available. These authors on a foundation of micro-anatomical study, correlating their histopathology with clinical observations, reach conclusions.

* These memoirs form, respectively, Nos. 142 and 178 of the Medical Research Council's series of Special Reports.

These conclusions are not wholly concordant; it would have been strange indeed if they had been for the subject is very complex.

We cannot hope to give in a few sentences a correct view of the structures which have been the object of study; perhaps the following hints are not hopelessly misleading. Functionally the kidney consists of an immense number of tubules each of which follows an intricate course from a blunt end into which fits a vascular tuft, called the glomerulus, to junction with a main drainage conduit. The minute structure of the cells lining this tubule differs in different regions. The blood supply is extremely rich, some enters at the tuft, leaves it by a vessel of smaller calibre and then passes through many minute vessels; part of the tubule receives a blood supply which has not passed through the tuft at all.

The rich and peculiar disposition of the blood supply suggested nearly a century ago to the English physiologist Bowman a theory that urine was produced by a double mechanism, filtration of the inorganic constituents at the tuft, and separation of the organic constituents lower in the tubule, a theory which still, in greatly altered shape, commands approval. Clearly the working of this machine may be thrown out of gear by the destruction of any of its parts. If the blood supply is disordered, the machine cannot work, grist is not brought to the mill, and the millstones—the living cells of the secreting tubules—may crumble. Even with a normal circulation, however, the tubules might sustain damage, for instance by the conveyance to them in the blood of toxins. Finally, in the kidney as elsewhere, the habit of "nature" of not replacing valuable articles when broken in a packing case but of putting in more and more packing material and so often breaking what valuables are still there, has full scope; fibrosis, provoked by some injury, may bind up the machine, altogether distorting its fine structure.

Modern micropathologists have followed out in detail the changes arising in different parts of this system and correlated them with clinical states.

The two recent memoirs cited above bear out the last paragraph. Russell's is the more detailed on the histopathological side, Gray's on the clinical, but in both histopathological evidence is primary. The two investigators do not agree on all points, but their disagreement is not of much importance to us. The main difference of opinion is as to whether a circulatory disorder, an ischaemia, unaided by an inflammatory change, is capable of producing a serious or fatal functional disorder of the kidney as an organ of excretion. Russell thinks it is not: Gray dissents. Any attempt by an outsider to sum up the arguments would be impertinent. To such an outsider the division between inflammation defined as "the local reaction of living tissue against a damaging agent", the presence of which justifies the term nephritis, and its absence which requires the term nephrosis (see Russell, pp. 119–20) is fine. But, whatever may be the best *nomenclature*, the experts recognize the existence of two groups of cases which

Gray terms kidney of essential hypertension and arterio-sclerotic kidney, of quite different clinical significance and not primarily of renal origin. The former group, characterized anatomically by widespread change in the arteriolar system, in the smallest arteries, includes serious cases, which clinically may exhibit even mortal signs of renal insufficiency. The latter group generally do not show signs or symptoms immediately due to disease of the kidneys.

We have singled out these groups because both of them would probably have been included by Bright as examples of his disease; certainly the essential hypertension group, probably the arterio-sclerotic group. A certifier of our day would include the whole of the second group and some proportion of the first group under arterio-sclerosis. *What* proportion would depend upon the prominence of renal signs and the personal idiosyncracy of the certifier.

Summarizing this summary—perhaps we should, more modestly, follow Calverley and say curtailing the already curtailed cur—we infer that research has restricted appreciably the pathological connotation of Bright's disease.

Passing from the pathology to the aetiology, using that term in a general sense, we have not derived much information from either memoir, for the sufficient reason that general aetiology was hardly within the authors' terms of reference. Gray's investigation was based upon 500 consecutive autopsies and some proportional frequencies are available.

Under his classification, there were 7 cases of acute nephritis, 6 of subacute or early chronic nephritis, 10 of chronic nephritis, 43 of kidney of essential hypertension, 15 instances of severe arterio-sclerosis and no less than 357 with *some* evidence of arterio-sclerosis.

Leaving out of account the cases of acute nephritis (also those of acute nephrosis) the relative frequency of kidney of essential hypertension is seen to be so great as compared with that of chronic nephritis, that its attribution to this or that class by a certifier must be of immense statistical importance. But what, if any, relation there may be between the lesions of the various types and antecedent habits or constitution cannot be decided. We note that in the hypertension group the patients whose cases are recorded in detail, where a uraemic element was grafted upon essential hypertension, were aged 38, 31, 34, 49 and 8½; only one then belonged to the middle period of life.

There may be somewhere a complete collection of the clinical histories of a large sample of patients. But then we should not have detailed pathological records of them because—a fact sometimes forgotten by statisticians—filling up schedules takes less time than cutting, staining and examining sections. Still one cannot escape a feeling that it *ought* to be possible to gain a little more knowledge of antecedents; actually we have opinions of very experienced physicians.

One of us many years ago extracted from the post-mortem books of the London Hospital the information recorded respecting persons aged 25 to 55 for the years 1889–1901. In order to form some idea of the change of nomenclature

the following experiment was done. We took out the first 300 and the last 300 of the records of males and noted the instances in which lesions of the kidney were recorded under cause of death. Taking the terms acute nephritis and parenchymatous nephritis to mean an acute condition, granular kidney, nephritis, chronic nephritis, interstitial nephritis, to mean a chronic condition, the results were these:

In the first 300 (post-mortems of 1889–91) acute conditions were recorded in 2 and chronic in 25 instances. In the last 300 (1900–1), acute in 1, chronic in 20. Restricting the record to those in which the renal condition was the *only* entry, the first 300 had 0 acute and 8 chronic disease, the last 300 had 1 acute and 4 chronic disease. There is perhaps a faint suggestion of decreasing importance of renal conditions. Arterio-sclerosis appears in the record twice in the first series, the case of a man of 43 said to have had degeneration of kidney, and the case of a man of 51 with cardiac thrombosis. It does not occur at all in the last 300.

It seems clear enough that Bright and his contemporaries attached a good deal of importance to excesses of drinking and eating, to metabolic vices such as gout, to acute diseases such as scarlet fever and to poisons such as lead in the production of the diseases they were talking about. To those earlier statements we owe the crystallizing out of what may be called the popular conception of Bright's, *chronic* Bright's, disease. The patient is a middle-aged man who has worked hard, worried a good deal, and done himself well. But we need not continue, newspapers still print advertisements of quack medicines.

Of course the advertiser had a professional model usually of an earlier generation. One of us has sentimental reasons for thinking tenderly of a novel published nearly 40 years ago by a medical practitioner who had been a student in the 'seventies. Its hero, John Armstrong, who stifled remorse for an early misdeed by prodigies of work and skill as an operating surgeon, first notices that something has gone wrong with his vision. He consults an ophthalmologist who suddenly asks: "Have you any great mental worry?" John starts: "Do you think I have Bright's disease?" Next John becomes unconscious in the middle of a clinical lecture and eventually dies, whether by accidental drowning in another fit or by suicide is left an open question. John Armstrong's creator would have certified either chronic nephritis or chronic Bright's disease. Dr Gray would have certified essential hypertension; the son of John Armstrong's creator, either chronic nephritis or arterio-sclerosis.

The pathological interpretation of the hard working and high living more or less remorseful business man has shifted from the kidney to the arterial system. We hope rightly, although there have been sceptics.

At the beginning of the twentieth century (in 1904) one finds a clinician of the older school, Georges Dieulafoy who had been a pupil of Trousseau and was the author of an immensely popular textbook, growing sarcastic: "Tout cela est fort bien, et ces notions concernant l'artério-sclérose sont du plus grand intérêt, mais

il faut convenir néanmoins qu'on a depuis quelques années singulièrement abusé de l'artério-sclérose; elle est devenue envahissante, elle veut tout expliquer, et dès qu'on éprouve quelque difficulté sur telle ou telle interprétation pathogénique et même clinique, on vous répond: c'est l'artério-sclérose!" (Dieulafoy, *Manuel de Path. Intern.* 14th ed., vol. I, p. 854.)

A material factor upon which the older writers put stress was, as we have said, the abuse of food and drink, particularly drink, and the association between Bright's disease and intemperance was a commonplace (John Armstrong fortified himself with "stimulants"). It is usually illustrated by the specific mortality of the trades in which the use of alcohol is likely to be considerable. For instance in the occupational analysis of 1910–12; i.e. of the period when the *statistical* mortality of nephritis was near the maximum, in the standard population 33 deaths out of 790 were attributed to Bright's disease for the population of all occupied and retired males. Publicans and spirit and wine dealers had 79 in 1265, inn servants 52 in 1173, barmen 63 in 1724. Angina pectoris and arterio-sclerosis (grouped together) had 9 deaths for all occupied and retired, 15, 15 and 29 for the groups named.

Present views of aetiology would probably have increased the attribution to arterio-sclerosis (*vide infra*). Now take groups in which the toll of Bright's disease is still more excessive, although the occupations involve no peculiar temptations to or facilities for using alcohol, file-makers and cutlers; the former had 165 deaths from Bright's disease and 26 from angina pectoris and arterio-sclerosis, the latter 64 and 20. Here the work of Gye and Kettle has made it probable that one is dealing with a directly toxic effect of silicic acid upon the renal epithelium as well as a general change. Much more of this occupational nephritis is primary than of the nephritis of the drink trades.

We must then separate the general aetiological factors of the heterogeneous group into two classes.

Overstrain, excess of food and drink, worry, would be held to contribute to the arterio-sclerotic form.

Bacterial infections or specific toxic substances, such as the industrial poisons of silicon, some anilin products, chromium and lead, belong to the aetiology of the primary renal form.

Can we form a general opinion as to whether all or any of these general factors have varied? So far as the factors of arterio-sclerosis are concerned there is no uniformity of belief. That the industrial risks, increased in specific instances by the discovery of a new process or at specific times as during the war, have *generally* decreased is almost certain. That seems as far as one may fairly go.

When we come to the statistics, we shall have a little more to say on the question of aetiology, but it will not amount to much. We pass now from our obviously imperfect attempt to indicate the best opinion of the time to the easier task of abstracting what was taught to beginners in textbooks. The facts—to

use the actuarial term—of the statistician are the opinions not of Richard Bright or his representatives in successive generations but those opinions strained through the minds of all the medical teachers and modified, for better or worse, by the minds of the taught and their individual experiences. The textbook writer lags behind the genius and the average statistical practice of the certifiers in any generation will lag behind the teaching of the textbook then current, because many of the certifiers were taught by the previous generation. Again, in a living art no mere textbook can reproduce the spirit in which it is taught. Still something can be learned from a perusal of books once famous.

THE TEXTBOOKS

In the generation immediately following Bright's work, say between 1840 and 1870, we believe the most popular treatise on medicine was that of Sir Thomas Watson. "Few medical works have been more successful than this", wrote Munk, "it has passed through five large editions, and has enjoyed a greater popularity with students and practitioners than any similar book since the First Lines of Dr Cullen" (*Roll of the College of Physicians*, vol. III, p. 292). Munk was writing about 60 years ago. The third revised edition of Watson's *Lectures on the Principles and Practice of Physic* bears the date 1848. Diseases of the kidney are discussed in vol. II, pp. 563 *et seq.*, 614–55. Watson describes the naked-eye appearance of Bright's kidney and continues: "And what are the signs which indicate, to an instructed eye, the presence of these changes [he is speaking of the chronic form]? Some of them are precisely the same, in kind, as those which denote the acuter disorder; only mitigated in degree, and of slower march and succession. The patients are subject to obscure lumbar pains; to sickness from time to time, and to retching; and their urine is apt to be red, brown or dingy, as well as albuminous, from the intermixture of the colouring matter of the blood. They are obnoxious to inflammations of the serous membranes also: and more particularly to head affections, of which, often, they die; drowsiness, convulsions, apoplexy. And, to finish the resemblance, many of them, aye most of them, become at length anasarcous." There may be no albuminuria; coma he would regard with Christison as the natural termination. Cardiac disease is often present and may, he thinks, be secondary to the renal condition, but he doubts whether cardiac diasease can produce renal disease. He cites, without expressing full agreement, Christison's four rules for the differential diagnosis of renal from cardiac dropsy, viz.: (1) Most cases of febrile dropsy are renal. (2) When in anasarca the oedematous parts are elastic and do not pit on pressure—Watson is clearly sceptical of this. (3) When the dropsy is attended by diuresis. (4) When the specific gravity of the urine is less than 1010.

We think that the effect on the ordinary student of this teaching would be to encourage the certification of deaths where the symptoms were cerebral and the signs of dropsy present as due to renal disease; no suggestion is conveyed that the

renal symptomatology might be secondary to disease of the circulatory system. It is worth reminding a reader that of the total number of certificated deaths, only a small proportion would be certified after a post-mortem examination.

We do not know whether any other textbook in vogue between, say 1860 and 1880, had the popularity of Watson's treatise. A teacher of that epoch who wrote a textbook much admired by one of our own teachers (the late Dr F. J. Smith) was Charles Hilton Fagge. We have consulted the edition of 1886 (*Principles and Practice of Medicine*, by the late Charles Hilton Fagge; London, 1886). Fagge's discussion (vol. II, pp. 429 *et seq.*) is anatomically much more detailed than Watson's. He regarded Bright's disease as a "diffuse non-suppurative affection of the kidneys requiring to be regarded as a substantive disease from the clinical point of view". His clinical picture is less vivid than but not greatly different from Watson's. He observes that much the most important group of cases shows cardiac symptoms, 17 per cent. or more, and remarks that "until recently cases of heart failure secondary to cirrhosis of the kidneys were almost always regarded during life as examples of a primary morbus cordis".

Fagge's teaching would hardly stimulate any transfer from the cardiac to the renal group of certifications.

From 1890 to the present day we can trace the changes of teaching in successive editions of one famous textbook, that of Dr James Taylor. In the first edition of the *Manual of the Practice of Medicine* (1890) there were signs of a change of view, indicated by the following remarks:

"As to the nature of the thickening of the arteries, very different opinions have been expressed" (p. 688). "The cause of the cardiac hypertrophy in renal disease has been no less hotly debated than the many other conditions in this interesting disorder." "By some, chronic Bright's disease, in the form of granular kidney, is regarded as a general and simultaneous affection of the heart, the arteries and kidneys; but if this were true, we still have to account for the precisely similar changes which occur in chronic parenchymatous nephritis in which case the renal disorder undoubtedly precedes the other symptoms."

Under atheroma (p. 507) he observes: "But such diseased vessels frequently coexist with Bright's disease", and in the preface he says it is doubtful whether Bright's disease should not be regarded as a general disorder.

Taylor leaned in his teaching to the view of the previous generation but a doubt is suggested. Twenty-one years later in the 9th edition (1911), the passage quoted above, beginning "By some", is retained, but we find the remark (p. 909): "Much more characteristic is the more or less uniform thickening (arteriosclerosis) which affects the small arteries all over the body, as well as the vessels of the kidney itself." But (p. 647): "The subject of the symptoms of arteriosclerosis presents the difficulty that since the condition is almost certainly brought about in most instances by the action of toxins or poisons circulating in the blood, which produce excessive tension in the vessels, the toxaemia and the

increased tension are as likely to be responsible for the symptoms as the structural change in the arterial walls." Still a note of scepticism, suggestive of the passage quoted earlier from Dieulafoy, but arterio-sclerosis has arrived.

Passing on another 20 years one reaches the 14th edition (1930). We now have a complete distinction between the secondary contracted kidney—secondary here meaning that the change is secondary to an inflammatory process in the kidney itself—and the primary contracted kidney, primary because the process is part and parcel of a general arterio-sclerotic change. In the former one will find a dilute urine containing albumen, little tendency to oedema, the exitus letalis will (using the old-fashioned nomenclature) be with cerebral symptoms. In the latter the urine will be normal save for a trace of albumen, there will be dropsy and exitus letalis with cardiac or other circulatory symptoms.

The student of this edition has every reason to certify many deaths which 30 years before would have gone to nephritis as due to arterio-sclerosis.

It is an interesting speculation to consider how a student with a knowledge of the 14th edition of "Taylor" would classify those of Bright's cases recorded to have had hard or contracted kidneys. Case 92, a man of 48 who died in an epileptic fit with convulsions whose heart is recorded to have been healthy, surely belongs to the secondary contracted group; case 24 of the woman aged 36 who died of anasarca presumably to the primary contracted group. One has the impression that a large proportion, perhaps a majority, would now have been classed to arterio-sclerosis and not to renal disease.

THE VIEWS OF MEDICAL STATISTICIANS

The reader is, we hope, now in the position of having *some* idea of the trend of scientific research and a rather clearer idea of the way in which knowledge was conveyed to the ordinary student of medicine. We pass now to the comments of the medical statisticians who annotated or drew inferences from the death certificates which came to the General Register Office. Of the successive commentators, Farr, Ogle, Tatham and Stevenson, Ogle alone could properly be said to have had much experience as a clinician and even his experience was limited to a comparatively few years during which he was assistant physician to a London teaching hospital. Farr had practised for a few years as a general practitioner; Tatham and Stevenson came to statistics from the Public Health Service. It is, on the whole, fair to say that, with the partial exception of Ogle, their knowledge of clinical medicine and pathology obtained after graduation was a book knowledge only.

Bright's conception of disease of the kidney already figured in the 1st Annual Report (see pp. 97, 105) and in the 2nd A.R. (p. 74) renal dropsy is mentioned. In the 4th A.R. a recommended nomenclature is printed, and renal dropsy and albuminuria are assigned to granular disease of the kidneys or *nephritis*, the term retained throughout Farr's period. In the 13th A.R. 430 deaths are assigned

to nephria and Farr remarks: "The apparent increase of the deaths by *Nephria* from 254 in 1847 to 430 in 1850 is partly referable to improvements in the diagnosis of the medical practitioners throughout the country: for the accuracy of these records depends as much on the diffusion of medical knowledge as on the progress of medical discovery. Nephria, as well as many diseases of the heart, were formerly referred to dropsy which is a common result of their protracted existence" (Appendix, p. 134).

In the 17th A.R. it is noted that the deaths assigned to nephria have reached 776 in 1854. In the 22nd A.R. (Appendix, p. 185) the increase, to 1258 in 1859, is thought to be due "to recent medical discoveries; some of the cases which were then (in 1850) classed under dropsy are now distinguished". In the 25th A.R. (Appendix, p. 189) we read: "some of these diseases nephritis and nephria (Bright's disease) increase largely; perhaps only in appearance, arising from a change due to the diffusion of pathological knowledge."

In both the 29th and 32nd A.R. the continued increases are noted. In the 32nd A.R. there is a reference to the "newly discovered cause of death *embolism*". In the 38th A.R. (p. 233) we read: "The diagnosis by chemical reagents, and as the result of pathological research, is more advanced now among not only the heads of the profession but among general practitioners. How much the increase of nephritis and the enormous increase of deaths referred to Bright's disease (nephria) is due to this cause it is difficult to decide." That concludes Farr's references to the matter.

He evidently attributed much of the increase to change of practice in certification. His successor, Ogle, had no doubt at all; on p. xvi of the Supplement to the 45th A.R. he writes: "There can be no reasonable doubt that the apparent increase of mortality from renal disease is attributable to the gradual extension of the knowledge of Dr Bright's discoveries and the recognition of cases as renal that previously were attributed to other causes. It is possible of course that there may also have been some real increase; but there is no evidence whatsoever that such has been the case." Ogle did not return to the subject, but in the 58th A.R. Tatham notes that the results of letters of inquiry when dropsy was the certified cause of death (a plan first begun in 1884) gave in 1885 attributions to the circulatory system and the kidneys in the ratio of 470 to 191, but in 1895 the ratio was 102 to 35. Farr had already noticed in the 'seventies that attributions of deaths to the circulatory system were increasing. Save for a passing reference in the 69th A.R. (p. cxiii), where the still continuing increase of mortality attributed to Bright's disease is attributed in part to improved certification, the question is not specifically raised again until the second decade of the twentieth century.

It is not, we think, unjust to say that Ogle was too sceptical of the value of statements on death certificates—the accuracy of which he took energetic measures to improve—to think attempts to evaluate increases or decreases of particular causes of death repaying, while Tatham was not much interested in

clinical statistics. Stevenson had the wide range of interests, the desire to find truth hidden behind figures, which characterized Farr so that after 1911 we enter on a new period of research. The first point to attract Stevenson's attention was the statistical importance of arterio-sclerosis, to which he devoted a critical study in the A.R. for 1916 (pp. lxxi–ii). He notes that in 1912 (the year following the first separate tabulation in our records) the Local Government Board gave much publicity to the subject of arterial disease, while in the same year the Registrar-General circulated suggestions to practitioners pointing out *inter alia* the inadequacy of "old age" as a certified cause of death. Deaths from arterio-sclerosis rose and the proportion per 1000 deaths at ages 75 assigned to old age fell from 346 in 1911 to 311 in 1912, 295 in 1913 and by 1916 to 267.

Although for arterio-sclerosis, so named, only six years' records were available, Dr Stevenson noted that a good indication of trend in certification was afforded by the heading "other diseases of blood vessels"; that heading included arterio-sclerosis of the present list and cerebral atheroma. The two made up in 1911 over 97 per cent. of the old heading Other Diseases of Blood Vessels. On this basis it seemed that the growth of mortality was from 38 per million in 1901 to 244 in 1916; the growth was particularly great after 1911.

Until 1901 nearly all deaths from diseases of cerebral arteries went to apoplexy. It was then customary to certify the condition resulting from cerebral haemorrhage; next a step back to the vascular lesion was taken; now a further step is made to the cause of the vascular lesion, arterio-sclerosis; a yet further step might be to pass from arterio-sclerosis to the cause of arterio-sclerosis, perhaps in some instances to Bright's disease and then again behind the kidney lesion to some toxin. In order to form some notion of the possibilities here, the deaths of males in London in 1914 assigned to arterio-sclerosis were tabulated by associated cause.

Of the 550, 218 mentioned only the arterial cause. Of the remainder, 47 recorded bronchitis and 29 chronic Bright's disease. Cerebral haemorrhage, apoplexy 84, other disease of heart 45. Cerebral haemorrhage then was losing more deaths than any other cause to the profit of arterio-sclerosis, although chronic Bright's disease lost 29, about 3 per cent. of the number actually assigned to chronic nephritis in London that year.

Ten years later (text volume of A.R. 1926, pp. 84 *et seq.*) Stevenson returned to the subject in an even more interesting essay. Now his principal object was to try to find a cause of death which might be an *index* of degenerative disease of the circulatory mechanism. He concluded that cerebral haemorrhage, taken in its widest sense, viz. to embrace thrombosis, embolism, etc., might serve. Unfortunately, he could not go back earlier than to 1901 because before then deaths certified to cerebral embolism or thrombosis went to the general heading embolism and thrombosis and could not be picked out. But basing himself on the quarter of a century's experience available, he came to the conclusion that the rate of

mortality was certainly not increasing, perhaps decreasing. He showed too that the increase of mortality from heart disease (confined to ages over 75 years) was fictitious, largely due to an increased habit of certifying myocardial degeneration in the aged.

He came decidedly to the conclusion that the increased mortality from both heart disease and from arterio-sclerosis was a matter of book-keeping and remarked: "So far, in fact, as the records of certification can show, alarmist pronouncements as to increase of mortality from heart disease by 'the stress and worry of modern life' may be met by the observation that it is declining at all periods of life."

Passing to the subject of chronic nephritis he quotes the standardized rates (*vide infra*) and remarks that: "from quite small dimensions this mortality had grown, presumably, to a large extent at least, as a result of increasing recognition, to a level in 1901–10 close to the maximum attained about 1914. This was succeeded by a very sudden fall, for males from 392 in 1915 to 288 in 1919, or 27 per cent. in four years, and for females from 287 in 1914 to 198 in 1919, or 31 per cent. in five years. Such a change in so short a period, and occurring between the dates mentioned, inevitably suggests the influence of war conditions, but if so this has been maintained since the peace, for since 1919 the rate for males has further fallen by 13 per cent. and that for females by 1 per cent." He points out a parallel fall in the mortalities attributed to alcoholism or to alcoholic nephritis. He suggests that: "the connexion between alcoholic excess and Bright's disease may be closer than might have been anticipated. Alcohol is commonly regarded both as a contributory cause of Bright's disease and as harmful to those who suffer from it. The latter fact may serve to explain why, if reduction of the supply of alcohol is assumed to account for the sudden fall in mortality from Bright's disease, this commenced as soon as the supply of alcohol was reduced, although its action in inducing the disease is presumably slow." He suggests that the continuance of the decline might be associated with the price of alcohol as one of the few wartime conditions still operative.

The statistics

We have now outlined the history of opinions held by certifiers and pass to the statistics compiled from their reports. Perhaps it will be said that, having regard to the history as related, manipulation of the data is idle. All they can give is a kind of numerical verification of changes of opinion. In Table I we have the statistically comparable figures for seven decennia. Here are included both acute and chronic Bright's disease. By 1891–1900 the rate has much more than doubled that of 1861–70, it rises a little more, then begins to fall, and by 1921–30 is appreciably less than it was 40 years earlier. The rise is entirely consistent with an increasing habit of certifying nephritis in preference to symptoms or signs —whether cerebral symptoms, coma, etc. or dropsy, etc.—and the fall is per-

fectly consistent with wider knowledge of ultimate aetiology leading to a re-transfer to arterio-sclerosis. Since the total rate of mortality from arterio-sclerosis is greater than that for nephritis, even allowing for an attribution to this group of a large number of cases which the older practitioners would certainly *not* have

TABLE I

Mean annual mortality per 1,000,000 *living. England and Wales. Acute and chronic nephritis*

Periods	All ages standardized	0–	5–	10–	15–	20–	25–	35–	45–	55–	65–	75+
Males												
1861–70	153	71	52	34	44	66	118	206	303	434	584	647
1871–80	269	135	91	49	66	94	179	337	545	844	1128	1234
1881–90	364	184	95	55	78	113	195	404	718	1214	1869	2116
1891–1900	418	174	77	50	73	103	182	420	844	1556	2396	2831
1901–10	435	148	67	47	64	96	162	367	837	1750	2822	3415
1911–20	406	121	62	57	72	96	154	310	732	1548	2789	3724
1921–30	303	67	42	40	61	78	98	200	484	1064	2217	3887
Females												
1861–70	95	48	31	25	33	56	87	134	169	252	321	265
1871–80	179	97	58	44	62	93	146	240	324	501	643	629
1881–90	265	152	68	51	75	119	184	333	483	798	1180	1196
1891–1900	308	143	55	51	68	108	183	367	604	1012	1564	1691
1901–10	325	122	54	50	65	96	164	349	633	1163	1813	2095
1911–20	302	111	54	53	66	97	152	294	565	1036	1757	2296
1921–30	255	55	34	47	58	90	125	216	486	827	1582	2515
Increase or decrease per cent. compared with 1861–70												
Males												
1871–80	+ 76	+ 90	+ 75	+ 44	+ 50	+ 42	+ 52	+ 64	+ 80	+ 94	+ 93	+ 91
1881–90	+138	+159	+ 83	+ 62	+ 77	+ 71	+ 65	+ 96	+137	+180	+220	+227
1891–1900	+173	+145	+ 48	+ 47	+ 66	+ 56	+ 54	+104	+179	+259	+310	+338
1901–10	+184	+108	+ 29	+ 38	+ 45	+ 45	+ 37	+ 78	+176	+303	+383	+428
1911–20	+165	+ 70	+ 19	+ 68	+ 64	+ 45	+ 31	+ 50	+142	+257	+378	+476
1921–30	+ 98	− 6	− 20	+ 18	+ 39	+ 18	− 17	− 3	+ 60	+145	+271	+501
Females												
1871–80	+ 88	+102	+ 87	+ 76	+ 88	+ 66	+ 68	+ 79	+ 92	+ 99	+100	+137
1881–90	+179	+217	+119	+104	+127	+113	+111	+149	+186	+217	+268	+351
1891–1900	+224	+198	+ 77	+104	+106	+ 93	+110	+174	+257	+302	+387	+538
1901–10	+242	+154	+ 74	+100	+ 97	+ 71	+ 91	+160	+275	+362	+465	+691
1911–20	+218	+133	+ 74	+112	+103	+ 73	+ 75	+119	+238	+311	+447	+766
1921–30	+168	+ 15	+ 10	+ 88	+ 76	+ 61	+ 44	+ 61	+140	+228	+393	+849

certified as Bright's disease, one has here quite enough material to make the nephritis rate of 1921–30 as great as that of 1901–10. A transfer of about 25 per cent. of the total mortality assigned to arterio-sclerosis would suffice. This would, of course, be a very crude method. The age distribution of certified deaths by Bright's disease is similar to that of deaths by arterio-sclerosis to this extent, that both show increasing rates with increasing age. But the increase is very

much steeper in arterio-sclerosis. A statistically negligible number of deaths is certified to arterio-sclerosis at ages under 45 and in the age group 65–75 the rate of mortality is only a third of that at ages over 75. Bright's disease is statistically responsible for few deaths under 45 but at 65–75 its mortality rate is more than half that at 75– and at 55–65 as much as 23 per cent. of it. Any method of re-allocation would have to take account of this difference.

To those unfamiliar with the material, it would seem that it *ought* to be possible by the sorting out of items to construct rates of mortality comparable from generation to generation. Let it be agreed that the group of illnesses which Bright first described was a mixed bag; still they at least had this in common, that they were mortal illnesses. The certifiers of the deaths will have entered some striking feature, symptom or physical sign. Why not assemble from the deaths certified to dropsy, apoplexy, cardiac disease, arterial disease, etc., etc., those clinically concordant with Bright's set, form rates of mortality year by year and draw a conclusion? The answer is that the titular subdivisions of causes of death as published, or indeed as retained in unpublished form, are not sufficiently minute to permit of the reconstruction. We have quoted above illustrations, such as the difficulty Stevenson himself found in tracing the antecedents of arterio-sclerosis as a cause of death.

Having given much thought to the subject, we are obliged to confess that we see no way of statistically eliminating, by new rates, the changes of opinion through 60 years and, therefore, cannot measure the share of that change of opinion in the moulding of the conventional rates. A frontal attack on the statistics seems to us hopeless.

Regional data

There remains for consideration the question whether by taking the data in flank we can force them to tell us the truth. Our national data are tabulated in many subdivisions; the geographical, administrative and even occupational unit of tabulation are employed. Suppose we have a subdivision of the data into a large number of groups and suppose further that the standard or practice of certification among the medical attendants of those dying in the groups is uniform then, if we make the group rates of mortality from various causes the primary object of study, it should be possible by the statistical method to reach some interesting results.

Let us begin with the most obvious of groupings, into town and country or, virtually into areas of high and low density of population. This is, of course, not really a simple division at all; townsmen and countrymen differ in many things other than housing density. Still, let us leave it at that for the moment. Now suppose we take out a scheduled cause of death A, and find that its rate is higher in town than country (we assume that obvious sources of fallacy, age and sex distribution, transfer of deaths, etc., have been eliminated). If the practice of

certification is really the same in town and country then it is a fair conclusion that the disease is really in some aetiological connexion with town life (whether by procatarctic factors or by selection is an open question).

In Tables II (*a*) and II (*b*) we show the rates of mortality from nephritis for areas in descending order of urbanization at the epoch of recorded maximal incidence and at decennial intervals thereafter. Confining ourselves to the

TABLE II (*a*)

Death-rates per million in age groups, and the standardized death-rates from acute and chronic nephritis, according to degree of urbanization in England and Wales. (*Males*)

Periods	0–	5–	15–	25–	35–	45–	55–	65–	75+	Total	Standardized death-rate
London											
1911–14	55	41	70	151	388	1065	2264	3751	5354	528	528
1920–22	53	32	65	108	248	554	1259	2614	4418	394	345
1930–32	33	27	71	103	181	495	1097	2641	5662	437	336
County boroughs											
1911–14	151	72	92	189	442	1092	2187	3534	3912	514	533
1920–22	103	50	77	138	259	601	1352	2338	3555	381	351
1930–32	50	38	82	103	210	560	1230	2783	4870	443	354
Urban districts											
1911–14	132	56	79	145	335	834	1731	3149	3757	436	440
1920–22	79	44	59	120	231	497	1158	2172	3409	354	309
1930–32	51	32	60	91	165	478	1057	2486	4582	408	310
Rural districts											
1911–14	100	47	70	105	238	573	1246	2478	3178	394	332
1920–22	62	46	51	100	157	367	892	1816	3172	331	252
1930–32	54	29	61	71	156	382	905	2346	4660	424	284
Death-rates expressed as percentage of those in 1911–14											
London											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	96	78	93	72	64	52	56	70	83	75	65
1930–32	60	66	101	68	47	46	48	70	106	83	64
County boroughs											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	68	69	84	73	59	55	62	66	91	74	66
1930–32	33	53	89	54	48	51	56	79	124	86	66
Urban districts											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	60	79	75	83	69	60	67	69	91	81	70
1930–32	39	57	76	63	49	57	61	79	122	94	70
Rural districts											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	62	98	73	95	66	64	72	73	100	84	76
1930–32	54	62	87	68	66	67	73	95	147	108	86

arithmetically more reliable rates of later age groups, it is seen that in 1911–14 there was an immense difference between the rates of London and the County Boroughs on the one hand and those of the rural districts on the other. In the

TABLE II (*b*)

Death-rates per million in age groups, and the standardized death-rates from acute and chronic nephritis, according to degree of urbanization in England and Wales. (*Females*)

Periods	0–	5–	15–	25–	35–	45–	55–	65–	75+	Total	Standardized death-rate
London											
1911–14	60	48	67	123	375	847	1459	2344	3128	421	379
1920–22	37	47	61	91	187	415	946	1611	2473	305	244
1930–32	20	31	75	88	159	390	935	1951	3657	395	262
County boroughs											
1911–14	159	66	88	146	406	850	1435	2164	2618	410	391
1920–22	76	50	62	105	223	457	953	1585	2084	299	254
1930–32	41	38	75	96	201	465	965	2014	3399	392	279
Urban districts											
1911–14	106	51	70	124	310	617	1249	2008	2445	355	324
1920–22	56	46	51	102	199	404	810	1467	2154	287	229
1930–32	37	38	69	88	175	385	809	1694	3097	357	242
Rural districts											
1911–14	97	44	68	115	234	453	862	1650	2191	319	257
1920–22	73	39	55	106	182	352	646	1350	2104	289	210
1930–32	46	30	66	87	173	353	797	1737	3270	381	241
Death-rates expressed as percentage of those in 1911–14											
London											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	62	98	91	74	50	49	65	69	79	72	64
1930–32	33	65	112	72	42	46	64	83	117	94	69
County boroughs											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	48	76	70	72	55	54	66	73	80	73	65
1930–32	26	58	85	66	50	55	67	93	130	96	71
Urban districts											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	53	90	73	82	64	65	65	73	88	81	71
1930–32	35	75	99	71	56	62	65	84	127	101	75
Rural districts											
1911–14	100	100	100	100	100	100	100	100	100	100	100
1920–22	75	89	81	92	78	78	75	82	96	91	82
1930–32	47	68	97	76	74	78	92	105	149	119	94

terms of Bright's original conception, this is what we should expect, another proof of

> O fortunatos nimium, sua si bona norint,
> Agricolas!

But it will be observed that at all important ages the town rates have declined more, usually much more, than the rural rates. Among males 55–65 the London rate of 1930–2 is only 48 per cent. of its value in 1911–14; but the rate in rural districts was still 73 per cent. of its maximum. Perhaps this does mean what we should like it to mean, viz. that improvement was greatest, aetiologically, where most needed. The factor to which Stevenson alluded, viz. a decreasing abuse of alcohol, must have been more potent in town than country. But, our King Charles's head of certification practice is still with us. It was Stevenson's opinion, and one could have no better opinion, that the precision of certification was greater in towns, particularly in London, than in country districts. A difference correlated with the greater resort to hospital and institutions.

Perhaps the greater improvement of the town rates is no more than the result of town certification being more precise, or at any rate more sensitive to changes of medical opinion. If so, then we cannot tell how much of the excess of town over country is due to greater precision of nomenclature on certificates. We need hardly add that this criticism must be restrained within limits. It has never been suggested by the most sceptical that country practitioners or informants miss deaths altogether; it is certain that the death-rate from all causes together at later ages is lower in country than town.

There is still something to be urged upon the sceptic. The difference between the experiences of males and females is interesting. In London, between 1920–2 and 1930–2, the rates on males continued to fall at all ages under 65 (except 15–20); on females they were rising at 65–75 as well as at 75+. The standardized rate in females was higher in 1930–2 than in 1920–2; in males it was lower. In the county boroughs the increase extends back to the age group 45– in females. In the urban and rural districts there is less difference. It is very hard to believe that local certification practice is different as between the sexes. Of course it can be argued, and not unreasonably, that the general aetiological factors—intemperance, etc.—of the arterio-sclerotic group prevail more in the male sex. That would account for a smaller transfer from the "true" nephritis group to the arterio-sclerotic group, but hardly for an increase in nephritis. Here is something needing investigation.

One also notes something worthy of investigation in the trend of mortality in early life, although here we are mainly concerned with acute nephritis. There is an increased incidence in the age group 15–25. We cannot in the regional data trace this into finer age groupings, but for the whole country it is possible (Table III). It will be seen that only at 15–20 for males but at both 15–20 and 20–25 for females there have been increases between 1920–2 and 1930–2. Is this a consequence of the wartime environment on early life?

TABLE III

The death-rates per million in age groups from acute and chronic nephritis in England and Wales for the two triennial periods, 1920–2 *and* 1930–2

	Age groups											
	0–	5–	10–	15–	20–	25–	35–	45–	55–	65–	75+	All ages
Males												
1920–22	81	50	41	53	81	122	231	513	1169	2174	3473	364
1930–32	49	30	35	63	76	92	180	489	1085	2558	4793	427
Females												
1920–22	64	46	47	47	68	102	203	413	836	1492	2157	293
1930–32	39	28	45	64	78	91	181	406	867	1833	3290	378

Occupational grouping

There is another method of grouping not subject, or not so greatly subject, to some of the difficulties just mentioned; the grouping by occupations. It is, of course, true that some large occupational groups, for instance agricultural labourers, are wholly non-urban workers. But if one considers the large number of separate groups, we have not the same constant bias. There is no obvious reason why the certification practice of the medical men who attend tailors should differ from that of those who attend weavers.

But it is certain that the occupational classification adopted in the successive censuses has changed so greatly that the contents of even the main groups now formed are occupationally different from those of 10 or 20 years ago. We think, however, that this objection does not weigh much against the use of the groups formed for the statistical purpose of measuring the correlation of mortality rates. We accordingly performed the following statistical experiment. In the 1910–12 occupational analysis, Bright's disease is a scheduled heading and another is angina pectoris and arterio-sclerosis.

In 1921–3 we have as separate headings chronic nephritis and arterio-sclerosis. If our reading of medical opinion is correct, Bright's disease of 1910–12 will include a much larger proportion of deaths belonging to that group of Bright's own cases in which a general factor of pathological changes is involved than the chronic nephritis of 1921–3. Again the angina pectoris and arterio-sclerosis of 1910–12 will be much less representative of the general factor than the arterio-sclerosis of 1921–3.

If then we take as a third variable deaths from all other causes than the two named, we should expect that in 1910–12 Bright's disease would be more highly correlated and arterio-sclerosis less highly correlated with other mortality than in

1921–3. The experiment was tried, using in each case all occupational groups for which at least 10,000 years of life were available. For 1910–12 there were 115 and for 1921–3 there were 119 groups fulfilling this condition. The results are shown in Table IV.

TABLE IV

1910–12 (*taking industries with population over* 10,000)

		N	Partial r
	S.E.		S.E.
All other causes and Bright's disease	$r_{12} = \cdot6407 \pm \cdot055$	115	$r_{12\cdot3} = \cdot647 \pm \cdot054$
All other causes and angina pectoris and arterio-sclerosis	$r_{13} = \cdot0846 \pm \cdot093$	115	$r_{13\cdot2} = -\cdot142 \pm \cdot091$
Bright's disease and angina pectoris and arterio-sclerosis	$r_{23} = \cdot2946 \pm \cdot085$	115	$r_{23\cdot1} = \cdot314 \pm \cdot084$

Subscripts: 1 = All other causes. 2 = Bright's disease. 3 = Angina pectoris and arterio-sclerosis.

1921–23 (*taking occupations with population over* 10,000)

All other causes and chronic nephritis	$r_{12} = \cdot4743 \pm \cdot071$	119	$r_{12\cdot3} = \cdot399 \pm \cdot077$
All other causes and arterio-sclerosis	$r_{13} = \cdot3984 \pm \cdot077$	119	$r_{13\cdot2} = \cdot296 \pm \cdot084$
Chronic nephritis and arterio-sclerosis	$r_{23} = \cdot3191 \pm \cdot082$	119	$r_{23\cdot1} = \cdot161 \pm \cdot089$

Subscripts: 1 = All other causes. 2 = Chronic nephritis. 3 = Arterio-sclerosis.

Their interpretation deserves a little thought. If we have correctly interpreted the trend of scientific opinion, the aetiological factors of arterio-sclerosis and of that form of kidney disease which is secondary to an arterio-sclerosis are *general* factors making for deterioration; those of a genuine primary lesion of the kidney are more particular, due to nocive agents of specific character. If then certification perfectly reflected the current state of knowledge, we should expect to find considerable correlation between rates of mortality from the arterio-sclerotic group and from all other causes, and but little correlation between the rate from chronic nephritis and all other causes (we mean by all other causes the death-rate obtained after exclusion of nephritis and arterio-sclerosis). But when certification does not perfectly reflect such knowledge, there will be considerable correlation between the mortality from nephritis and from other causes and also considerable correlation between the mortality from arterio-sclerosis and from chronic nephritis. This will arise because in a group in which the arterio-sclerotic type of renal disease is prevalent, some practitioners will assign a death to arterio-sclerosis, others to chronic nephritis.

Now consider the arithmetical results. In the 1910–12 period, the partial correlations show a considerable association between Bright's disease and other causes, no significant association of arterio-sclerosis and other causes and a significant association of Bright's disease and arterio-sclerosis. In 1921–3, the

association of chronic nephritis with other causes is appreciably smaller, that of other causes with arterio-sclerosis appreciably larger and the association of chronic nephritis with arterio-sclerosis negligible.

We have in fact moved appreciably nearer to the anticipated position. It is unfortunate that the data of 1930–2 are not yet available for a further test. One can only speculate as to whether the chronic nephritis–other causes correlation will have declined still more and the arterio-sclerosis–other causes correlation increased again.

We do not, of course, expect that the chronic nephritis–other causes correlation will vanish. Even if all the really arterio-sclerotic kidney diseases pass to the arterio-sclerotic group, there must remain a link which will produce an arithmetical effect due to a genuine pathological common factor.

If the nephritis mortality rates in occupations are scrutinized in the light of our general knowledge, one sees that excesses fall into two groups. On the one hand, we have those groups in which an excessive use of alcohol is socially probable, the occupations concerned with the sale or manufacture of alcoholic drinks.

Actually the correlation between mortality from cirrhosis of the liver and

TABLE V

	Cirrhosis of liver		Chronic nephritis	
	C.M.F.	Ratio	C.M.F.	Ratio
Cellarmen	45·1	4698	66·6	1930
Brewers of ale, stout and porter	76·8	8000	55·9	1620
Inn-hotel keeper and publican	110·9	11,552	78·1	2264
Barmen	56·0	5833	88·7	2571
All occupied and retired	9·6	1000	34·5	1000

The C.M.F. (Comparative Mortality Figure) provides means of comparing the mortality at ages 20–65 experienced in different occupations, allowing for differences between the age distributions of the populations in those occupations. A standard population is chosen in such a way that the death rates at ages from all causes of all occupied and retired males would yield precisely 1000 deaths in that standard. The relative positions of other occupations are shown by the corresponding number of deaths reached when their death rates at ages from all causes are applied to the same standard population. The expected deaths are similarly found for different causes by applying the death rates at ages from the selected cause to the standard population. For example, the recorded death rates from cirrhosis of the liver experienced at different ages by all occupied and retired males produce 9·6 deaths in the standard population; the death rates of barmen produce 56·0 deaths. The total mortality of barmen is, therefore, 6 times the mortality of all occupied and retired males. This relative position is more clearly shown in the column headed ratio in which the C.M.F. of all occupied and retired males is taken as 1000 and those of other occupations expressed as ratios of that figure.

chronic nephritis can be brought out on the whole of the data. In the official report 162 occupations were used for correlation and the correlation of the comparative mortality figures of the two causes was shown by the Registrar-General to be $0{\cdot}419 \pm 0{\cdot}044$. Here we imagine one is dealing with the arterio-sclerotic form of nephritis. But one has a wholly different group of occupations in which the certified incidence of chronic nephritis is high.

TABLE VI

	Population	C.M.F.	Ratio
Rag grinders	3,556	77·2	2238
Pottery dippers and glaziers	2,117	79·5	2304
Coppersmiths	4,106	94·2	2730
Wool spinners and piercers	6,854	99·9	2986
Cotton and blow-room operatives	2,722	102·9	2983
Tin and copper miners (underground workers)	1,775	118·6	3438
File-cutters	1,425	215·1	6232
All occupied and retired	9,704,860	34·5	1000

The population is the number of workers between the ages of 20 and 65 years recorded in these occupations at the 1921 Census.

Here, for instance in pottery dippers and file cutters, we have occupations in which the toxic factors of lead and silicon are plainly of importance. In others, notably the textile groups, the aetiology is obscure. The two branches of textile workers have a mortality from nephritis three times greater than the average for all occupied and retired males. The Registrar-General in the Occupational Supplement for 1921–3 has described the position in the textile industry as follows: "the textile position is still more remarkable in regard to chronic nephritis, from which not one of the sixteen occupations fails to return mortality in excess of the average. With this nephritis excess, no doubt, is associated another distinctive mortality of textile workers—that from cerebral haemorrhage. From this cause only two of the sixteen textile occupations, wool sorters and wool weavers, fail to exceed the average mortality. It thus appears that the conditions of work in textile mills promote degenerative changes of the kidneys, heart, and blood vessels."

What are the incriminating factors responsible for these conditions? Does the type of shed, dry or humid, in which the cotton weavers work tend to promote these degenerative changes? The mortality occurring in moist and dry sheds, as given in the Occupational Mortality Supplement for 1921–3, is certainly suggestive, but, of course, one cannot assert that artificial humidity was the sole factor. The death-rates from cerebral haemorrhage and chronic nephritis in towns in which artificial humidity is used in the sheds are higher than those in which it is not used. On the other hand the mortality from circulatory disease is

considerably lower in wet sheds than in dry ones. The history of the mortality during 1921–3 was as follows:

TABLE VII

Disease	Cotton weavers in towns where artificial humidity in the majority of sheds			
	Was used		Was not used	
	C.M.F.	Ratio	C.M.F.	Ratio
Cerebral haemorrhage	62·1	1383	48·4	1078
Circulatory disease	110·5	726	245·8	1615
Chronic nephritis	45·0	1304	32·7	948
All causes	1065·0	1065	834·0	834

The ratio was obtained by relating the C.M.F. value for the particular disease to the C.M.F. value for the same disease amongst all occupied and retired males. Thus for Cerebral Haemorrhage the ratio was $\frac{62{\cdot}1}{44{\cdot}9}$ or 1383 and for All Causes $\frac{1065}{1000}$ or 1065.

One has here an interesting unsolved problem.

This completes our study of the statistical history of Bright's disease. We chose the subject partly because it is of interest in itself, partly as a characteristic example of a recorded rate of mortality in which it was *a priori* certain that changes of opinion had been an important factor of statistical variation. It seemed to us desirable that some pains should be devoted to a task unlikely to lead to brilliant discoveries. A topic of the same class, but much more hackneyed, is the share of changing opinion in the increase of mortality from cancer. We do not know that this subject has been investigated on the lines of our paper, viz. by bringing into relation with the statistics a history of opinion.

The practical conclusions to which we are led are these:

(1) We do not think that the statistical data of different generations can, by any practicable reclassification, be rendered sufficiently comparable to permit of any sound inferences as to whether the factors favouring "Bright's disease" did really increase or decrease through the last 60 years.

(2) We think that by 1921–3 a sufficient degree of uniformity in practice of certification had been reached to admit of statistical study of group mortality rates having some aetiological significance.

(3) A corollary of (2) is that the data of our own time will repay analysis by medical statisticians.

(4) A medico-statistical analysis of hospital data, bringing the clinical and personal particulars into still closer relation with the histopathological data, now so far more complete than in former times, would be of great value.

THE LONDON SKULL

By MATTHEW YOUNG, M.D., D.Sc.

1. *Introduction.* To Mr Warren R. Dawson belongs the merit of having recognized the importance of the fossilized fragment of a human cranium found during the excavations for Lloyd's new building in the city of London in 1925. On his advice the members of the Committee of Lloyd's submitted the specimen to Prof. (later Sir Grafton) Elliot Smith for scientific examination, and later very generously presented it to the Anatomical Museum of University College, London, where it is now readily accessible for inspection by anyone interested.

Mr Dawson has written the following account of the circumstances in which the fossil was discovered:

During 1924 and 1925 the site bounded by Leadenhall Street, Lime Street, and Leadenhall Place in the City of London, upon which the historic East India House originally stood, has been excavated for the erection of a new building for the Corporation of Lloyd's. Before the erection of the steel-work began, the central part of the site was cleared by means of mechanical excavators; but as the stanchions and girders rendered the available space more and more restricted, part of the digging had to be done by manual labour. The chance of finding fossil bones was slight in that part of the area in which the steam excavator was used; for this apparatus raises large masses of earth at each plunge and deposits its burden bodily into iron skips, which are in turn hoisted by cranes and emptied into lorries. On such parts of the site as were dug out by manual labour, however, fossil bones have from time to time come to light.

By the kind permission of the Committee of Lloyd's, facilities have been given for a scientific examination of these bones, and the clerk of the works, Mr G. T. Murton, has in all cases carefully noted the exact depths and the nature of the soil in which the finds were made. In March 1925 I exhibited three specimens at a meeting of the Zoological Society. These comprised the head of a femur and some molar teeth of the mammoth (*Elephas primigenius*), found in the river gravel at depths of 20 and 37 ft. respectively, and the ulna of a rhinoceros, which in Mr M. A. C. Hinton's opinion, may provisionally be referred to the species *R. antiquitatis* Fischer. This ulna came from the redeposited London clay, which at this spot underlies the gravel at a depth of 40 ft. It was actually found at a depth of 42 ft. These specimens have already been described and figured (Warren R. Dawson & M. A. C. Hinton, *Proc. Zool. Soc. Lond.* 1925, Part 2, p. 793). The rhinoceros bone has been presented to the British Museum by the Committee of Lloyd's.

At a later stage in the excavations some further remains came to light. Amongst these were the antlers and some limb-bones of the red deer (*Cervus elaphus*) from the river gravel at a depth of 30 ft., and the greater part of the skull of an ox from another part of the site at the 26 ft. level. The most interesting fossil, however, is part of a human skull, recovered from the blue clay, the same formation as that in which the remains of the woolly rhinoceros were found, and at exactly the same depth, 42 ft., but in the western portion of the site. The fragment of skull was broken into four pieces by a blow from the excavator's pick; and one of the pieces, a small triangular splinter from the anterior border of the parietal bone, was not recovered. The other three pieces were fitted together and exhibited at a meeting of the Zoological Society of London on 20th October. On that occasion the erroneous statement was

made that the skull was found at a depth of 26 ft. from the surface; but a few days later the clerk of the works directed my attention to the error and informed me that the human fossil came from the blue clay in the 42 ft. level.

The misunderstanding arose from the fact that our inquiries concerning the "skull" were believed to refer to the remains of the ox found at a depth of 26 ft., and not to the flattened plates of bone, which were not recognized as parts of a skull.*

2. *Probable Age of the Skull.* The most probable age of the deposit in which the human remains were found has been the subject of much discussion. Mr M. A. C. Hinton,† of the British Museum, is convinced that it must be assigned to the third or lowest and latest of the terraces of the Thames, that generally known as the 20 ft. terrace containing the characteristic late Pleistocene fauna and the implements of later palaeolithic man—an Aurignacian horizon—but Mr C. N. Bromehead,‡ of the Geological Survey, is of the opinion that the fluviatile beds which underlie Leadenhall Street form part of the Taplow or Middle or 50 ft. terrace.

At Sir Grafton Elliot Smith's request Miss Dorothy A. E. Garrod examined the evidence for the age of the skull, and her final report which we are permitted to quote is as follows:

The opinion of Mr H. Dewey, of the Geological Survey, when I first consulted him about the age of the deposit was on the whole in favour of Mr Bromehead's opinion, since borings from the neighbourhood of the Lloyd's site showed the surface of the London clay, which forms the bedrock of the terrace, at an average height of 30–35 ft. O.D. Since that date, however, further work has been done on these deposits, and Mr Dewey has very kindly given me the result of his recent researches. Fresh investigations of borings in the immediate neighbourhood of Lloyd's have largely contradicted the older findings. Mr Dewey thinks the reason for this to be that many of the bores were started from basements, and in one particular case where this is now known to have happened it is found to give an error of 20 ft. in the O.D. height of the London clay. The general result of recent work has been to place the surface of the London clay in this area from 9–14 ft. O.D., and therefore to suggest that the deposits underlying Lloyd's do in fact belong to the Flood Plain Terrace, in spite of the fact that aggradation has brought their surface up to 50–55 ft. O.D. Mr Dewey tells me that he feels sufficiently sure of his ground to alter the mapping of the Leadenhall Street deposits, and to mark them as Flood Plain in the forthcoming survey of the London area. This being so, it remains to examine its bearing on the age of the London skull, which can no longer be considered as contemporary with the Taplow Terrace. Unfortunately, no implements were found in the redeposited clay of the Lloyd's section, but the fauna leaves no doubt that it is Pleistocene, and that it was laid down in a period of cold. In the Admiralty section, where the Flood Plain deposits were well exposed, Mr Lewis Abbott found Upper Palaeolithic implements in the uppermost part of the fluviatile beds. By the courtesy of Mr Abbott I have been able to examine these, and I have no doubt that they are Aurignacian. Their presence in the upper part of the Flood Plain gravels does not, however, prove that the whole of the Flood Plain deposits are necessarily post-Mousterian. In the valley of the Somme, M. Commont found Levalloisian implements in gravels lying just

* This account is taken from Sir Grafton Elliot Smith's communication on the London skull in *Nature*, 1925, Vol. 116, p. 678.

† *Proc. Zool. Soc. Lond.* 1925, Part 2, p. 793.

‡ *Nature*, 1925, Vol. 116, p. 819.

above river level, these gravels being capped by a deposit with Aurignacian tools. In the Thames Valley, however, no implements older than Aurignacian have so far actually been found in Flood Plain deposits, and it would be unwise to postulate for the Lloyd's skull an age more remote than the Upper Palaeolithic.

Mr K. P. Oakley, of the Department of Geology, British Museum, is of the opinion that Miss Garrod's report adequately sums up the position. He has very kindly given the writer permission to supplement this report by the following statement which he has prepared, and in which he summarizes the most recent views (June 1937) on the age of the stratum in which the skull was found:

A Taplow (Middle Levalloisian) age does not seem to be entirely ruled out by the revised level of the London clay bench, but even if the skull occurred in deposits belonging to the lower part of the Upper Flood Plain Terrace, there remains the possibility that it is of Late Levalloisian (Mousterian) age (see W. B. R. King & K. P. Oakley, "The Pleistocene succession in the lower parts of the Thames Valley", *Proc. prehist. Soc.* 1936, pp. 65–7), so that in conclusion one may say that the presumptive age of the skull must be at least Aurignacian, and very possibly older (Middle to Late Levalloisian).

Other authorities are not in agreement with Miss Garrod's view. In November 1925, Mr J. Reid Moir* expressed the opinion that the London skull was in all probability to be referred to the Mousterian epoch. In August 1932† he made the following further comments on the probable age of the skull: "The blue clay in which the specimen was found is well known and widespread over East Anglia. It is to be referred to the second inter-glacial epoch of that region, is usually surmounted by the upper chalky boulder clay, and is to be seen at High Lodge, Mildenhall, Hoxne and at Ipswich. In each case mentioned the clay contains unabraded examples of either late Acheulean or early Mousterian flint implements."

Sir Arthur Keith‡ is of the opinion that the evidence collected by geologists and by students of man's palaeolithic tools indicates a far greater antiquity for the London skull than that which has been assigned to it by Mr M. A. C. Hinton. He says: "the evidence is most definite that the gravel bed which covered the London skull was laid down before the Acheulean culture had reached its last phase and long before the Mousterian culture had begun", and that the skull should be considered to belong to the earlier and not to the later palaeolithic epoch.

While there is every reason for believing that the fragment of skull was naturally deposited and formed part of a human being who was a contemporary of the woolly rhinoceros, its age must be assumed to be at least Aurignacian, and very possibly older. In spite of differences of opinion on its exact age, the skull is undoubtedly that of the earliest Londoner yet discovered, and is thus of

* *The Times*, 4 November 1925.

† *Ibid.* 17 August 1932.

‡ *New Discoveries relating to the Antiquity of Man*, 1931, pp. 437 and 443.

sufficient interest to merit a detailed description of its characters and an inquiry into its affinities with other types.

3. *Brief Statements of Expressions of Opinion on the Skull by Sir Grafton Elliot Smith and Sir Arthur Keith.* The conclusions reached by the late Sir Grafton Elliot Smith from his examination of this skull are incorporated in two communications on the specimen to *Nature** and *The British Medical Journal*† in 1925, and in his essay on "The Human Brain"‡ published in 1927. In his view the skull reveals interesting primitive traits. The sagittal contour appears to be exceptionally flat and of distinctive outline. The endocranial cast, not only in the outline of its sagittal contour, but also in the modelling of its surface, shows some resemblance to the corresponding characters in the casts of female Neanderthal skulls from La Quina and Gibraltar, though the form of the cerebellum and the slightly greater fullness of the cerebral surface in the parietal region seems to indicate that the skull belonged to a rather primitive type of the species *sapiens* and was not Neanderthaloid. Sir Grafton also gave a detailed account of the evidence in support of his firmly held belief that the "Lady of Lloyd's" was left-handed.

Sir Arthur Keith§ is of the opinion that the London skull represents a modification of the human stock first revealed to us at Piltdown, Sussex. Its anatomical characters are those seen in the skulls of Piltdown and modern types of humanity, but its nearest affinity is to the Piltdown type.

4. *Anatomical and Morphological Features.* The principal anatomical and morphological features of the skull may now be described in some detail. The fragment includes the greater part of the occipital and left parietal bones and a portion of the right parietal. The figures in Plates I and II illustrate its general appearance from the lateral, vertical, occipital and internal aspects.

The portion of the occipital bone that is present comprises practically the entire squama occipitalis with the exception of a small part in the region of the left occipito-mastoid suture and a segment that appears to be relatively narrow, though this cannot be asserted positively, which formed the posterior boundary of the occipital foramen. The upper part of the squama to the extent of almost a third of the supra-inial arc is formed by a pre-interparietal bone, the transverse line of demarcation between which and the lower part of the squama is quite easily traced. The external occipital protuberance is fairly well defined but not very prominent, and the moderately arched superior curved lines which extend from the inion towards the lateral angles of the bone are very slightly elevated and rounded in their medial parts but fade away as they are traced laterally. The

* *Nature*, 1925, Vol. 116, p. 678.
† *Brit. Med. J.* 1925, Vol. 2, p. 854.
‡ *Essays on the Evolution of Man*, 2nd ed. 1927, p. 176.
§ *Op. cit.* pp. 463–467.

surface of the squama below the line presents relatively faintly developed muscular impressions. The right and left segments of the lambdoid suture that limit the bone laterally can be traced in their whole extent but are completely synostosed except in the lower fourth. The portions of the lambdoid suture bounding the pre-interparietal section of the bone laterally deviate appreciably from the curves of the lower and more extensive parts of the suture, so that the lambda lies farther forward than might be expected if the bone were of the normal type. On the endocranial aspect of the fragment, the line of junction of the occipital and parietal bones is clearly indicated at the lambda and as far down as the lower limit of the pre-interparietal segment of the bone by a definite groove, but it is not apparent below this level. The groove for the superior sagittal sinus is continued into the groove for the right lateral sinus as usually occurs. The level of the internal occipital protuberance is practically coincident with that of the inion, unlike the arrangement usually found in Neanderthaloid specimens. There is a definite asymmetry of the posterior cerebral fossae, the right being deeper than the left. This reversal of the normal asymmetry has been fully dealt with by Sir Grafton Elliot Smith in his discussion of the evidence of left-handedness.* The cerebellar fossae are exceptionally well defined and are separated by a prominent internal occipital crest. The crest terminates abruptly in a fractured surface, apparently at the point whereat it divides in most skulls into the two diverging ridges that limit laterally the small triangular area lying behind the occipital foramen. From a comparison of this feature with the corresponding features of complete skulls, it is possible to reconstruct tentatively the defective portion of the occipital bone, and so obtain the approximate position of the opisthion or midpoint of the posterior margin of the occipital foramen.

The left parietal bone is nearly complete but is deficient to some extent in its anterior area at the antero-superior and antero-inferior angles. A small wedge of bone has also been lost just below the midpoint of the anterior border. No part of the coronal suture is visible on the outer or inner surface of the most projecting parts of the anterior margin of the bone, but it may be assumed, with a fair measure of confidence, that the coronal line of fracture coincides approximately with the position of the suture from its relationship to the groove for the middle meningeal vein which is present on the endocranial aspect and is situated about 1 cm. behind the tip of the lower projection. This tip probably reaches to within 1 mm. of the coronal suture. On the sagittal border of the bone near its midpoint a short section of the sagittal suture is preserved. The lower border of the bone is intact in the great part of its extent. The section forming the parieto-mastoid suture line is practically complete. The portion articulating with the squamous part of the temporal is preserved in a sufficient part of its normal extent to give an indication that the parieto-squamous suture line was probably moderately well arched. The superior temporal line can be easily traced, though

* *Essays on the Evolution of Man*, 2nd ed. 1927, pp. 176–189.

not specially prominent, and appears to be situated relatively higher up on the parietal than in the average modern skull. The parietal eminence is not obtrusive. On the inner aspect of the bone the grooves for the anterior and posterior divisions of the meningeal vessels are clearly indicated. The thickness of the parietal bone on measurement at several points is found to be approximately 5 mm. on the average, i.e. much the same as the modern English female skull.

The portion of the right parietal bone that is preserved is a triangular area of which the base is formed by the lambdoid suture, and it comprises probably just under one-third of the original area of the bone.

The bones are of a reddish-brown colour and seem to be very heavily mineralized so far as can be judged from their weight, general appearance and the nature of the surface along the lines of fracture. The high degree of mineralization of the parietal bone is shown clearly in Plate III, where its radiograph is placed beside that of a parietal bone of much the same thickness belonging to a seventeenth-century Londoner's skull of the Whitechapel series. The condition of the cranial sutures (lambdoid and sagittal) suggests an age of more than 40, but probably less than 50 years. The smoothness of the contour of the skull, and especially the faintness of the muscular impressions in the occipital region, make it highly probable that the sex is female.

In its anatomical and morphological characters there is no clear evidence that the skull, though apparently relatively low and flat, should be considered of primitive type. The presence of the pre-interparietal bone has hitherto been considered a recently acquired feature which is relatively common in modern skulls but not seen in skulls of palaeolithic age. It may be noted, however, that a closely corresponding anomaly of the occipital bone is shown in the Saccopastore* skull, a female specimen belonging to the Neanderthal species, which was excavated near Rome and is of well-authenticated Mousterian age.

5. *Comparison of the London Skull with other Types.* In an attempt to throw further light on the affinities of the London skull, the measurements and relative proportions of the cranial characters that were determinable in the incomplete specimen have been compared in detail with the corresponding characters, where available, in the following skulls or groups of skulls: (*a*) a number of upper palaeolithic female skulls from European sites, (*b*) British skulls, including the Bury St Edmunds fragment, of reputed late palaeolithic or earlier date (excluding the Piltdown and Swanscombe specimens), (*c*) Seventeenth-century London and modern Scottish series, (*d*) female Neanderthal skulls from Gibraltar, La Quina and Saccopastore, (*e*) the Piltdown skull, and (*f*) the Swanscombe skull.

(*a*) *Comparison with the Solutrean and Aurignacian Female Skulls.* The comparison of the measurements and their relative proportions in the London fragment with the corresponding characters in the female upper palaeolithic

* Sergi, Sergio, *Rivista di Antropologia*, 1934, Vol. 30.

(chiefly Aurignacian) group is shown in Table I. It will be seen that in this relatively short series considerable variation is shown in the majority of the characters which are tabulated. Though no real emphasis can be laid on the mean values of the several characters as being stable or truly representative of the series, these have been tabulated for the characters measurable in the London skull and other important cranial characters which may be estimated approximately in this specimen. The characters in which the London skull deviates most notably from the corresponding characters in the Aurignacian series as a whole can be seen from the last column in Table I. On the assumption that the variabilities of the several characters in the Aurignacian group may be represented approximately by the standard deviations of the corresponding characters in the Scottish female series, the difference between the measurement of each character in the London skull and the mean value of the corresponding character in the Aurignacian group has been divided by the appropriate standard deviation. For the characters in which the ratio of the difference to the standard deviation is under 2·0 the London skull cannot be said to differ to a significant degree from the Aurignacian series. A survey of the figures in the last column shows that there are only six characters in which the ratio exceeds the value 2. These are S_2, S'_2, $100 \times S'_2/S_2$, $100 \times$ biasterionic B/S'_2, the height of the parietal arc and the parietal arc height index. All of these characters are associated with the length of the parietal bone or its curvature. Comment will be made later on the relatively short and flat parietal bone which, if our reconstruction at the bregma and the orientation of the skull can be relied upon, is a peculiar feature of the London skull.

Though we have taken the female skulls associated with the Aurignacian and Solutrean cultures together as a group, which is referred to, for brevity, as the Aurignacian series, it is more instructive to compare the London skull with the individual skulls in the group.

If our estimates of the maximum length (L) of the London skull as approximately 180 mm. and the basio-bregmatic height as relatively low, probably even as low as 120 mm., are accepted, while the maximum breadth (B) is known to be at least 144 mm. and not more than 146 mm., it is obvious that in size and proportions the London skull seems to differ very appreciably from many of the Aurignacian female skulls. The skulls which agree most closely with the London skull in length are No. V from Solutré (1924) and that from Obercassel. The Obercassel skull is, however, much narrower and probably much higher than the London skull, and the comparison with the London skull may generally be restricted to the Solutré skulls. Solutré I is appreciably longer and rather less wide but definitely higher than Solutré V, the basio-bregmatic heights being 132 and 123·5 mm. respectively. The Solutré V skull is undoubtedly relatively low and relatively wide in form. Its cephalic index is 81·0. Assuming that the estimate of the length of the London skull as 180 mm. is approximately accurate,

TABLE I

*Showing the comparison of the Principal Measurements of the London Skull with the corresponding Measurements in Female Upper Palaeolithic Skulls found in Europe**

Characters	The London skull	Cro Magnon No. II	Solutré I (1923)	Solutré V (1924)	Grotte des Enfants (Cro Magnon type)	Grotte des Enfants (Negroid type)	Predmost No. IV	Predmost No. X	Obercassel	Lautsch II	Le Placard (1881)	La Vallée du Roc No. I	Means	$\frac{\Delta}{S.D.}$ †
Maximum length (L)	—	192·5	185·5?	182·0	[195·0]	191·0	191·5	185·0	182·0	[182·0]	175·0	185·0	185·5 (9)	—
Maximum breadth (B)	144?	138·0?	146·0?	147·5	140·0??	130·5	143·5	140·5	129·5	132·0?	140·0	140·5	138·9 (11)	1·1
Basio-bregmatic height (H')	—	—	132·0?	123·5?	[120·5]	133·5	136·5	—	134·5	—	128·0	129·0??	131·0 (7)	—
Auricular height (OH)	—	[111·0]	115·0	[110·0]	—	114·5	[117·0]	106·5	111·5	[118·0]	—	—	111·9 (4)	—
Biasterionic breadth (B''')	114	—	115·5?	116·0??	—	—	—	104·0??	112·0	—	108·0	109·2?	110·8 (6)	0·8
Parietal arc (S_2)	105	131·0?	124·0??	110·5?	121·0?	134·5	132·5	115·5?	139·0	132·5?	118·0	133·0	126·5 (11)	−3·4
Occipital arc (S_3)	123	—	133·0??	130·0?	—	118·0	123·5	122·0	111·0	—	119·0	—	122·4 (7)	0·1
Lambda-inion arc ($S_{3\cdot1}$)	82	—	86·0??	85·5	[108·0]	71·0	73·5	70·0	63·0	—	—	54·0	71·9 (7)	1·3
Inion-opisthion arc ($S_{3\cdot2}$)	41	—	47·0	44·5	—	47·0	50·0	52·0	48·0	—	—	—	48·1 (6)	−1·4
Parietal chord (S' 2)	101	123·0?	114·2?	102·9?	111·1?	120·9	119·7	105·7	125·0	117·5?	—	—	115·6 (9)	−3·1
Occipital chord (S' 3)	100	—	104·2?	99·1?	—	98·4	100·6	96·4	94·6	—	—	—	98·9 (6)	0·2
Lambda-inion chord ($S'_{3\cdot1}$)	76	—	74·9	77·2?	[94·9]	66·4	68·1	65·9	59·7	—	—	—	68·7 (6)	1·1
Inion-opisthion chord ($S'_{3\cdot2}$)	40	—	46·1	42·9		45·1	48·9	50·3	46·1	—	—	—	46·6 (6)	−1·3
Height of parietal arc	12	23·0	19·0	18·0	21·0	26·0	24·0	20·0	27·0	—	—	—	22·3 (8)	−4·4
Height of lambda-inion arc	15	—	16·0	13·0	20·0	12·0	11·0	11·0	8·0	—	—	—	13·0 (7)	0·9
Lambda-inion-opisthion angle	117°	—	118°·0	108°·0	[110°·0]	120°·0	117°·0	112°·0	126°·0	—	—	—	116°·8 (6)	0·1
Length-breadth index (100 B/L)	—	71·7?	78·7?	81·0	—	68·3	74·9	75·9	71·2	[72·5]	80·0	75·9	75·3 (9)	—
Height-breadth index (100 B/H')	—	—	110·6?	119·4?	—	97·8?	105·1	—	96·3	—	109·4	108·9??	106·8 (7)	—
Index of parietal curvature (100 S'_2/S_2)	96·2	93·9?	92·1??	92·1??	91·8?	89·9	90·3	91·5?	89·9	88·7	—	—	91·1 (9)	2·9
Index of occipital curvature (100 S'_3/S_3)	81·3	—	78·3	76·2	—	83·4	81·5	79·0	85·2	—	—	—	80·6 (6)	0·4
Pearson's occipital index	58·1	—	56·0?	54·9?	—	60·1	58·2	56·4	62·3	—	—	—	58·0 (6)	0·1
Lambda-inion chord/arc	92·7	—	87·1	90·3	—	93·5	92·7	94·1	94·8	—	—	—	92·1 (6)	0·3‡
Inion-opisthion chord/arc	97·6	—	98·1	96·4	—	96·0	97·8	96·7	96·0	—	—	—	96·8 (6)	0·5‡
Lambda-inion arc/occipital arc	66·7	—	64·7	65·8	—	60·2	59·5	57·4	56·8	—	—	—	60·7 (6)	1·2
Inion-opisthion arc/lambda-inion arc	50·0	—	54·7	52·0	—	66·2	68·0	74·3	76·2	—	—	—	65·2 (6)	−0·9
Inion-opisthion chord/lambda-inion chord	52·6	—	61·5	55·6	—	67·9	71·8	76·3	77·2	—	—	—	68·4 (6)	—
Parietal arc height index	11·9	18·7	16·6	17·5	18·9	21·5	20·1	18·9	21·6	—	—	—	19·2 (8)	−4·4
Lambda-inion arc height index	19·7	—	21·4	16·8	—	18·1	16·2	16·7	13·4	—	—	—	17·1 (6)	1·0
Biasterionic B/maximum breadth	79·2	—	79·1	78·6	—	—	—	74·0	86·5	—	77·1	77·7	78·8 (6)	0·1
Biasterionic B/parietal chord	112·9	—	101·1	112·7	—	—	—	98·4	89·6	—	—	—	100·5 (4)	2·5
Biasterionic B/occipital chord	114·0	—	110·8	117·1	—	—	—	107·9	118·4	—	—	—	113·6 (4)	0·1

* The measurements of characters in the individual upper palaeolithic skulls are taken from the memoir by G. M. Morant in the *Annals of Eugenics*, 1930, Vol. 4.

† The standard deviations used here are those for a series of 47 modern Scottish female skulls given in Table II. Δ represents the deviation of the measurement for the London skull from the upper palaeolithic mean.

‡ See footnote § to Table II.

with a breadth of 144 mm. the cephalic index of the specimen would be 80. If the basio-bregmatic height were as low as 116 mm.—which it might possibly be on the assumption that it holds the same proportion to cerebral height (i.e. the vertical distance from the subcerebral plane* to the vertex) as is shown in the Scottish series—the length-height index would be $100 \times 116/180 = 64{\cdot}4$. If the height were 120 mm. the length-height index would be 66·7, compared with 67·9 in the Solutré V skull. A basio-bregmatic height of 120 mm., or even 2 or 3 mm. greater, appears to be more probable than one less than 120 mm. The higher value is strongly supported from the relationships shown in the superimposition of the sagittal contour tracings of the London and Solutré V skulls (Fig. 1), to be referred to later. The London skull is thus probably very similar in its general form to the Solutré V skull.

On comparison of the other characters in the two skulls, we find that in biasterionic width the Solutré V skull is approximately equivalent to the London skull. Its parietal bone is almost as short as the estimated length in the London specimen, as is shown by the parietal chords of 102·9 and 101·0 mm. and the sagittal parietal arcs of 110·5 and 105·0 mm., respectively. The parietal curvature in Solutré V appears, however, to be rather, though not greatly, in excess of that estimated for the London skull, as shown by the respective ratios of chord to arc, 92·1 and 96·2. The lambda-opisthion (occipital) arc is even longer in the Solutré V and Solutré I skulls than in the London skull. Their upper segments (the lambda-inion arc) are relatively long as compared with their lower segments (the inion-opisthion arc). In this respect these two skulls resemble the London and differ strongly from all the other Aurignacian female skulls. The lambda-inion and inion-opisthion chords in Solutré V are of much the same size, both absolutely and relatively, as in the London skull. The corresponding characters in Solutré I are not greatly different from those in Solutré V, but in this respect it deviates more from the London skull than Solutré V does. The curvatures of the two segments of the occipital arc, as represented by the ratio of the chords to the arcs, in Solutré V are closely akin to those in the London skull. The ratio of the lambda-inion arc to the total occipital arc, and, as might be expected, the ratio of the two segments of the arc to one another, also agree fairly closely in the two skulls. Solutré V shows some divergence from the London skull in one character; its occipital bone as a whole is more curved. This divergence is brought out by an angle and two indices in the table. The lambda-inion-opisthion (occipital) angle is less obtuse in the Solutré V, being 108° as compared with 117° in the London, the index of occipital curvature ($100\, S_3'/S_3$) is 76·2 as compared with 81·3, and Pearson's occipital index in Solutré V is 54·9 as compared with 58·1 in the London skull. In regard to total occipital curvature Solutré I is nearer the London than is Solutré V. Three ratios

* For a detailed definition of the subcerebral plane, first used by Sir Arthur Keith, see the account given by him in *The Antiquity of Man*, 1916, p. 379.

are tabulated of which the biasterionic breadth is a component. The proportions which the biasterionic breadth shows to the maximum breadth and to the sagittal extent of the parietal chord in Solutré V are almost identical with the corresponding ratios in the London skull, while the ratio of the biasterionic breadth to the occipital chord (S'_3) is only slightly greater in the specimen from Solutré than in the London, the respective ratios being 117 and 114. So far as can be judged from the available measurements, the London skull would appear to be indistinguishable in type from the skull Solutré V.

The sagittal contour tracings of the London skull and the Solutré V skull are superimposed in Fig. 1. As neither the subcerebral plane nor the Frankfort plane was readily identifiable in both skulls (the first being determinable approximately in the London skull and the second alone indicated in the tracing of the Solutré skull) and since the bregma-inion chords were found to be identical, the method adopted was to make the two bregmas and the two inions coincident. The slight divergence in the two contours is brought out in this way, the London skull being fuller in the lower occipital and less full in the parietal region than the Solutré skull, though the divergence in the latter region may be to some extent accentuated by the deficiency that exists in the vault of the Aurignacian specimen from France.

(*b*) *Comparison with the British Skulls, including the Bury St Edmunds Fragment, of reputed Late Palaeolithic or Earlier Date, but excluding the Specimens found at Piltdown, Sussex, and Swanscombe, Kent.* Comparison of the London skull with upper palaeolithic female skulls has been mainly confined to those associated with this phase of culture found on the continent, as few well-authenticated specimens of the period have yet been found in England. A brief reference will be made here to the general features of the London fragment and to those in which the specimen resembles or differs from some skulls of late palaeolithic or earlier date found in England. Sir Arthur Keith gives a good summary of the general characters of these skulls.*

It must first be noted that if the estimated length of the London skull as 180 is approximately correct—the breadth being 144 or 146—then the cephalic index is 80 or 81, so that the skull must be considered of brachycephalic type although at the lower limit of the range. With a length of 180 mm. and a basio-bregmatic height probably about 120 mm., the altitudinal index would not be much greater than 67, so that the skull is definitely chamaecephalic.

Of the five human skulls which have been discovered in Aveline's Hole in the Mendips by the Spelaeological Society, University of Bristol, associated with an Azilio-Tardenoisian culture, two fall into the round category, their width being at least 80 per cent of their length, while three are relatively long. According to Keith, this is the earliest evidence of a brachycephalic people in England. All the skulls have, however, unlike the London specimen, a characteristically lofty

* *New Discoveries relating to the Antiquity of Man*, 1931.

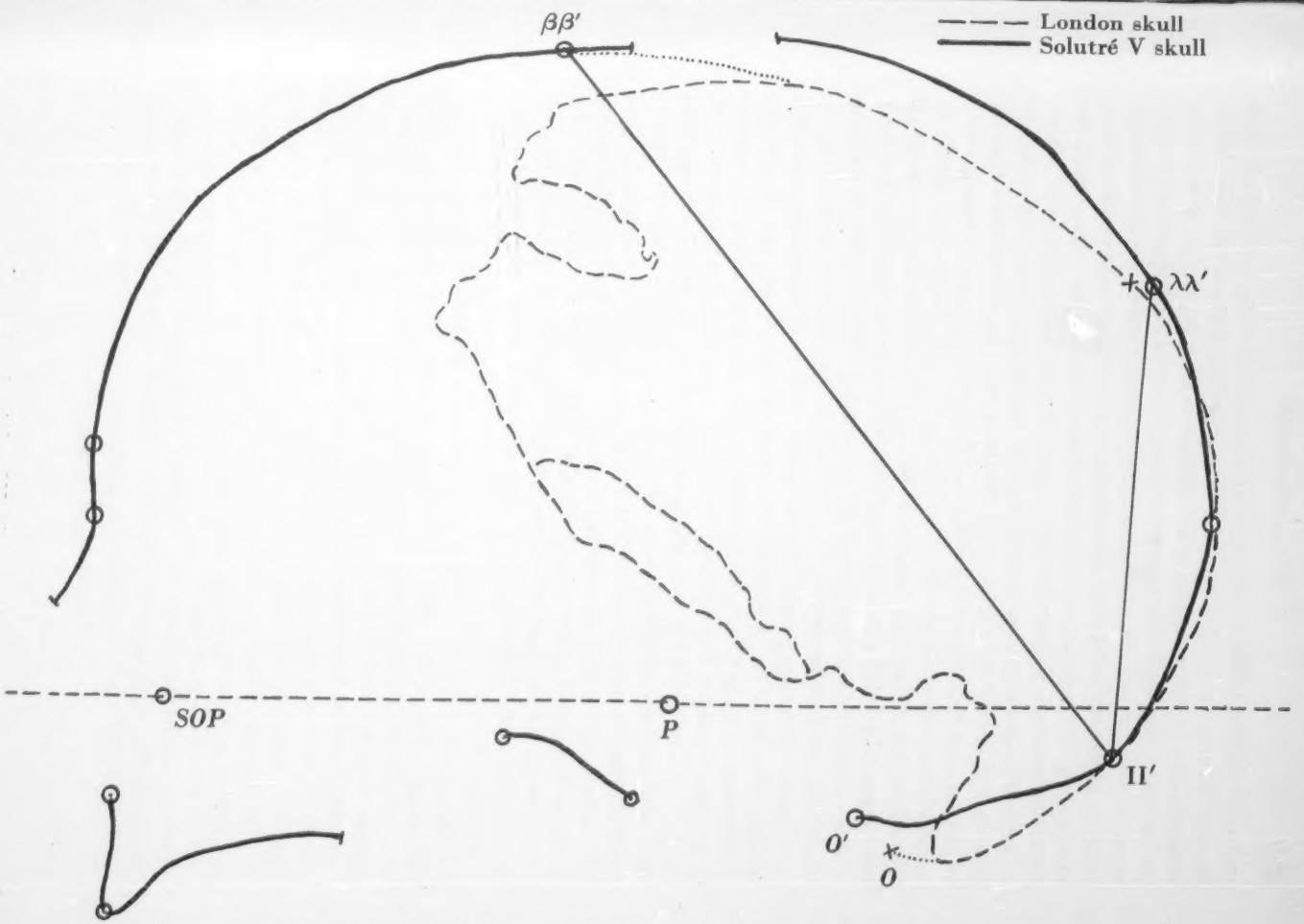

Fig. 1. Superimposition of the median sagittal tracing of the London skull on that of the Solutré V skull taken from Morant's memoir. The bregma-inion chord in the two skulls is identical and this chord has been made coincident.

vault. The famous Cheddar male skull which was found in Gough's cave in 1903 has a cubic capacity of 1450, definitely larger than that estimated for the London specimen. The length is 196 mm., the breadth 138 mm., the cephalic index 70·4, the height of the vault above the earholes 115 mm. and that above the subcerebral plane 105 mm. In all its characters the skull conforms to the river-bed type. In 1928 remains of five other skeletons were found in the same cave. Three of these were juvenile and one adult was represented only by a lower jaw. The remaining specimen was the calvaria of a young man, probably under 25 years of age, with an estimated cranial capacity of 1425 c.c., i.e. little different from that in the Cheddar No. 1 skull. With a length of 192 mm. and a breadth of 144 mm. the cephalic index of Cheddar No. 2 is 75, i.e. just within the long category and relatively broader than Cheddar No. 1. The vault is definitely lower, however, in Cheddar skull No. 2 than in No. 1, the respective subcerebral heights being 95 and 105 mm. The height of the vault of the skull Cheddar No. 2 is thus very little in excess of that of the London skull. All the skulls found so far at Cheddar are dolichocephalic.

Another specimen of special interest is that found in a fragmentary condition at Kent's Cavern, Torquay, in 1925. It is most probably of early post-glacial age. It is that of a young female, probably under 25 years of age. As reconstructed at the Museum of the Royal College of Surgeons, its estimated length is 175 mm. and maximum breadth 143 mm., giving a cephalic index of 81·7. The woman was thus definitely brachycephalic with an estimated cubic capacity of 1400 c.c. The skull had the same high vault as those found in the Aveline's Hole specimens, the highest point of the vault rising 120 mm. above the earholes. In this feature it differs characteristically from the London skull.

A brachycephalic skull is also said to have been found in the deposits at Cresswell Caves, Derbyshire, belonging to the same cultural stage as that shown at Aveline's Hole.

There is thus definite evidence of the presence of skulls in England in late palaeolithic times with length-breadth proportions of brachycephalic type, corresponding to that estimated for the London skull; but they are all definitely higher in the vault than it is.

Comparison of the London skull fragment with another cranial fragment found in England is of peculiar interest because, though the remarkable correspondence in certain of their features may merely be a coincidence, it is so striking as to suggest the possibility that it may be of special significance. The specimen in question is the Bury St Edmunds fragment, which can, according to Keith, be referred with a considerable degree of confidence to the later Acheulean phase of culture, immediately preceding the Mousterian phase to which, if not earlier, the London skull is believed possibly to belong by some recent investigators. Mr J. Reid Moir* is of the opinion that this skull, found in clay at Westley near

* *The Times*, 17 August 1932.

Bury St Edmunds and sometimes referred to as the Westley skull, is almost certainly of the same age as the London skull. An account of the fragment was published by H. Prigg* in 1885, but a more detailed account of its characters, accompanied by photographs of the different aspects oriented in relation to the subcerebral plane, was published by Keith† in 1912. The fossilized fragment of skull which is preserved in the Moyses Hall Museum, Bury St Edmunds, has been examined by the writer, but its comparison with the London skull fragment in this paper is mainly based on the photographs of the different aspects published by Keith in his memoir. The outlines of these were enlarged to life-size to facilitate comparison. The fragment consists of the upper two-thirds of the frontal bone and the anterior third of the right and left parietal bones. The upper region of the forehead presents a sharp "frontal bend". At the bend the frontal bone is comparatively thin and it is preserved intact sufficiently far forward to convince Keith of the practical impossibility that on such a forehead great simian eyebrow ridges were implanted. The characters of the specimen, according to Keith, clearly indicate a person with a head of the modern type, of the female sex (judging from the shape of the forehead and probable size), and probably over 40 years of age (judging from the condition of the sutures). Sir Arthur Keith made an attempt to reconstruct the probable outline of the missing parts of the skull and found its prototype in a skull, showing the same fronto-parietal contour, obtained from a gravel deposit in the East End of London and of uncertain antiquity. He used this as a guide in estimating the probable measurements of the missing parts. From the drawings of the lateral and upper aspects of the skull fragment when oriented in the subcerebral plane, Keith came to the following conclusions. After allowing for the missing parts of the frontal and parietal bones and the absent occipital bone, the length was probably 183 mm. It may have been shorter but not longer. The vault was remarkably flat, a character in which the Bury St Edmunds fragment resembles Neanderthal skulls. This flattening of the vault is probably natural and not due to soil pressure. Judging from the width and flattening of the vault the original transverse diameter of the skull could not have been less than 148 mm., the width being thus 80 or 81 per cent of the length. Such a skull would be classified as brachycephalic, but it is of a totally different type from most modern brachycephalic skulls, since the vault is so low. At the utmost the height of the vault above the ear holes could not have been more than 105 mm. The estimated brain capacity of such a skull, using the Lee-Pearson formula

$$(183 \times 148 \times 105 \times 0{\cdot}4 \times 206 = 1340 \text{ c.c.}),$$

is about equal to that of a modern Englishwoman. The Bury St Edmunds fragment, according to Keith, "is such a mutilated document that one may well

* *J. Anthrop. Inst.* 1885, Vol. 14, p. 51.

† *J. Anat. and Phys.* 1912, Vol. 47, p. 73.

hesitate in forming any certain conclusion as to the type of person it represents". A similar inference might reasonably be drawn from a study of the London skull fragment, and yet when the outlines of the lateral aspects of the two fragments are superimposed, making the bregmatic points and the coronal sutures coincident, it seems difficult to attribute what immediately becomes evident purely to chance. The superimpositions of the tracings of the lateral and upper aspects are given in Figs. 2 and 3.

Fig. 2 shows the Bury St Edmunds skull fragment, superimposed on the London skull fragment in such a way that the bregmatic points and the lines that indicate the sagittal contour tracing and the coronal suture tracing diverging from these points are all coincident. It will be recollected that the positions of the bregma and the coronal suture in the London fragment are only estimates, but they are estimations which can, from the extent of the parts preserved, be made with a fair degree of accuracy. When this superimposition is made it is found that the contour tracing of the Bury St Edmunds fragment is in almost exact agreement with the contour of the frontal region in the London skull which the writer had predicted as most probable from a knowledge of the measurements and their proportions in the London fragment. The most probable contour outline of the missing parietal and occipital region predicted by Sir Arthur Keith from the nature of the existing Bury St Edmunds fragment is rather less full in the occipital region than the contour as completed by the London fragment. Keith estimated the length of the parietal bone along its curvature as 126 mm., slightly greater than the frontal. The predicted position of the lambda is rather lower than in the London skull, but if allowance had been made for the fact that there is a definite inverse association between the lengths of the occipital and parietal bones, and that a long occipital arc that usually occurs when a pre-interparietal is present is likely to be accompanied by a relatively short parietal, the difference in the occipital region of the London skull and the predicted occipital outline of the Bury St Edmunds fragment by Keith would have been in still closer agreement. When the fragments are superimposed in the way described, the line of the subcerebral plane in the London skull—which can be estimated very approximately from the left asterion and the inion—exactly coincides with the subcerebral plane which Sir Arthur Keith had inserted in his reconstructed drawing for the Bury St Edmunds fragment. The subcerebral plane passes through the posterior inferior angle of the parietal and as a rule through, or just above, the fronto-malar junction. When a skull is so placed the highest point of the vault is situated in the majority of cases about 8 mm. above the level of the bregma and about 40 mm. behind it. It was on this principle that Keith oriented the Bury St Edmunds fragment. In any manner of orientation and measurement the vault of the Bury St Edmunds skull is a low one: the highest point of the vault is only 91 mm. above the subcerebral plane. This estimate coincides almost exactly with the corresponding estimate of the height

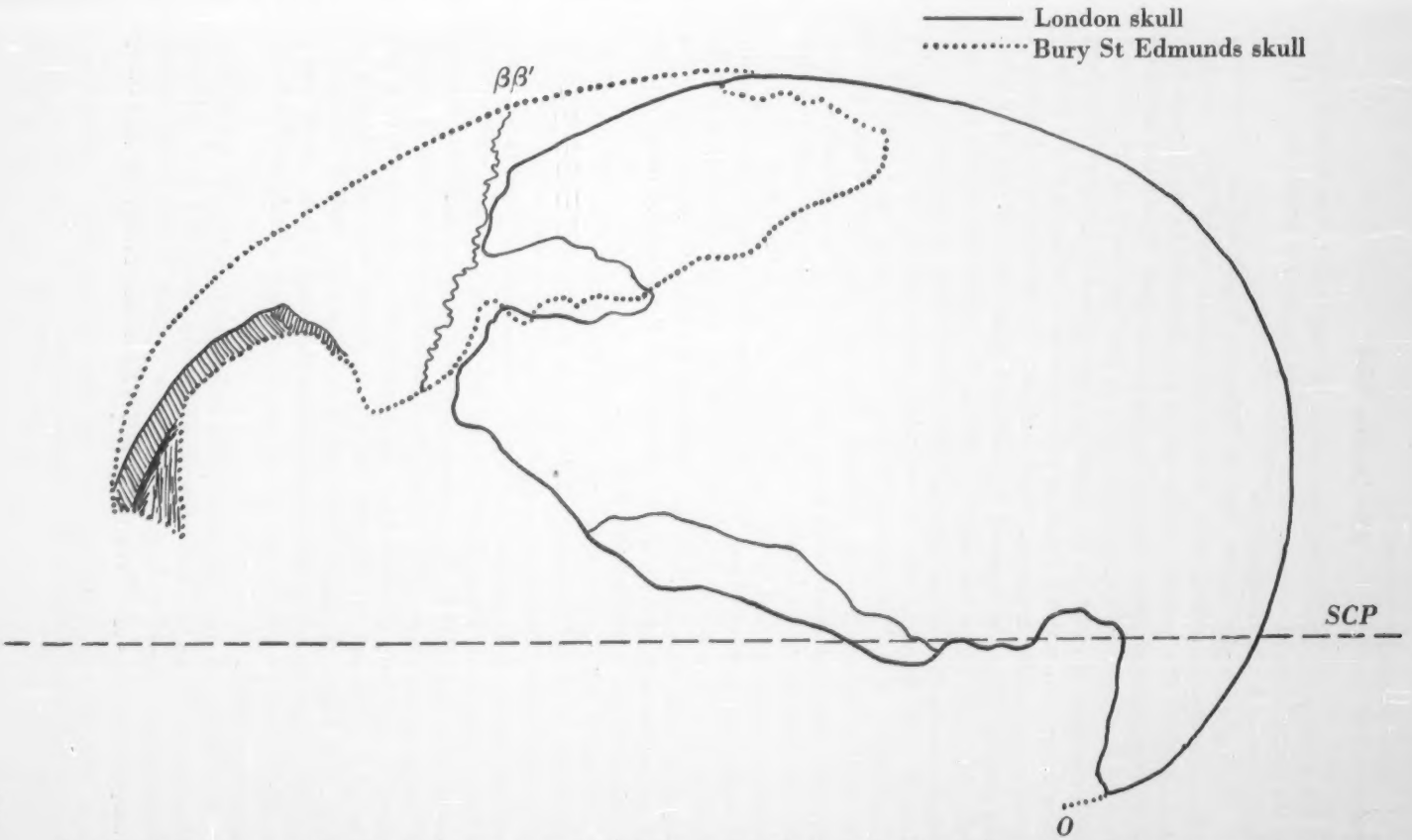

Fig. 2. Superimposition of median sagittal tracings of the London and Bury St Edmunds fragments with the bregmas and the adjacent corresponding parts of the coronal and sagittal sutures made coincident.

of the vault of the London fragment above the same plane. The slope or angle of the coronal suture is of assistance in verifying the plane of orientation and the angle is set as in the modern type.

Keith gives a view of the Bury St Edmunds fragment from above when oriented in the subcerebral plane, with an approximate outline of the complete skull indicated. The projected outline was largely determined from a comparison of the fragment with crania showing the same characters. This writer estimates that the maximum width of the skull could not have been less than 148 mm., and the proportion of width to the length is thus estimated to have been about 80 or 81 per cent., which is practically the same as our estimate of the cephalic index in the London fragment. Keith estimates the interstephanic diameter of the Bury St Edmunds fragment to have been about 110 mm. (the stephanion being the point where the temporal line for the attachment of the temporal muscle crosses the coronal suture). The corresponding measurement on the London skull fragment can be estimated approximately by doubling the measurement on the left side, and it seems to be of much the same order as the estimate for the Bury St Edmunds fragment. Fig. 3 shows the Bury St Edmunds fragment viewed from above when oriented in the subcerebral plane and superimposed on the London skull fragment oriented in the same plane; the bregmatic points as well as the sagittal and coronal sutural lines in the two tracings being made coincident. The close approximation of the London fragment contour to the contour completed by Keith for the Bury St Edmunds fragment is very striking; in the two specimens the estimated lengths are almost the same; the estimated maximum breadths and the estimated interstephanic breadths are also approximately the same.

We must fully acknowledge the risk of inaccuracy that is entailed in attempting to predict, even approximately, from such relatively small fragments as are preserved of the London and Bury St Edmunds specimens the characters and dimensions of the missing parts, yet in either case the predictions of the missing parts in one fragment are in remarkably close agreement with what is extant in the other. While this similarity may merely be a coincidence, it seems rather, in our view, to support the thesis that the two fragments represent calvariae that were essentially similar in type, being practically of the same length, about the same width and consequently showing an almost identical proportion of breadth to length, or cephalic index, which is at the lower limit of the brachycephalic range. So far as can be judged from the remains, both cranial vaults showed the same flattened contour and were equally low in their relation to the subcerebral plane.

(c) *Comparison of the Characters of the London Skull with those of* (1) *a Racially Homogeneous Group of Female Seventeenth-Century Londoners*, (2) *a Relatively Homogeneous Sample of forty-seven Modern Scottish Female Skulls without Interparietals, of a Type closely akin to that of the London Group, and* (3) *a Group*

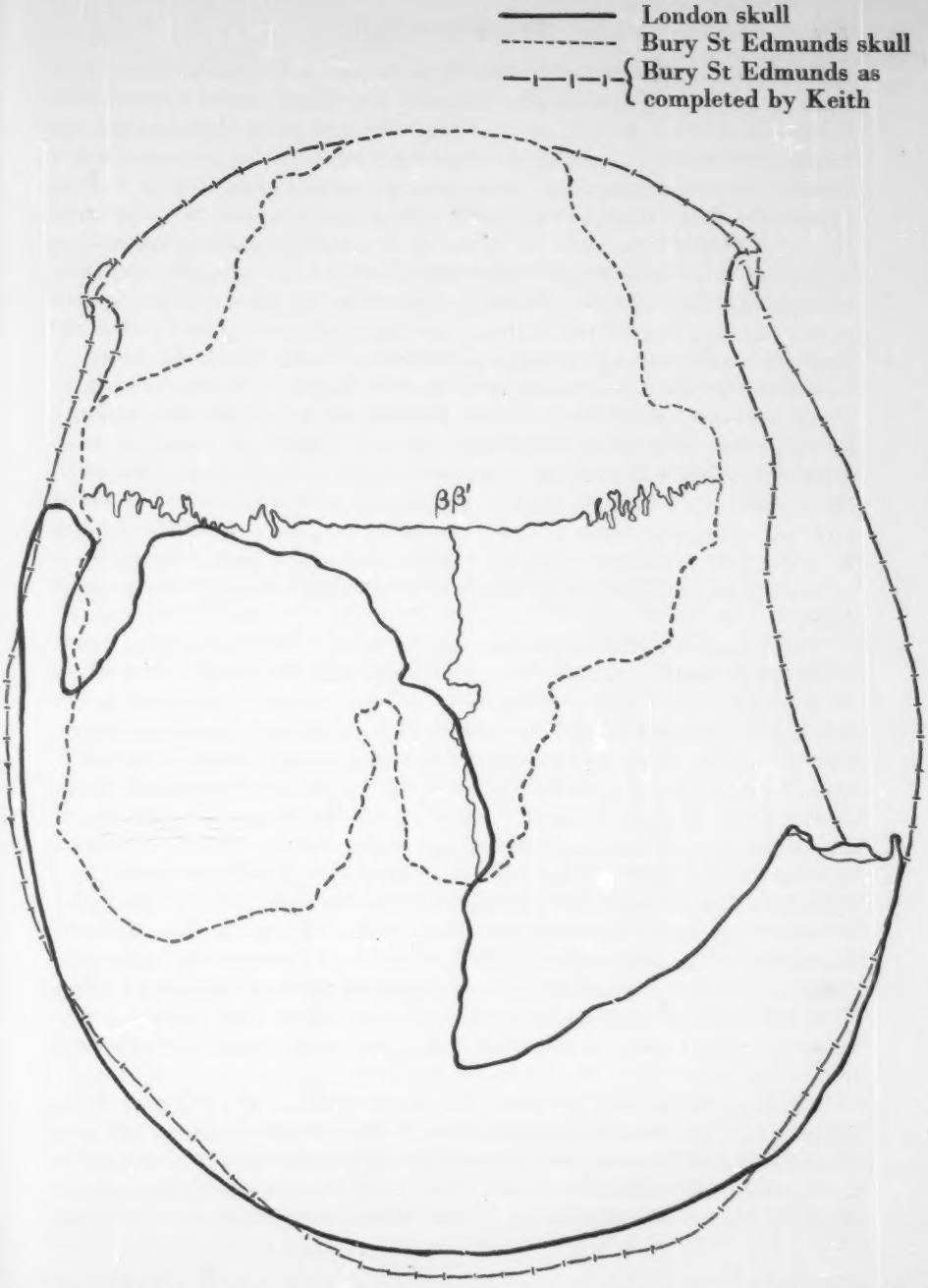

Fig. 3. Superimposition of tracings of the London and Bury St Edmunds fragments seen from the upper aspect with the bregmas and the adjacent corresponding parts of the coronal and sagittal sutures made coincident. The skulls are oriented in the subcerebral plane.

of ten Female Skulls with Interparietals from the same Scottish Series. The several features in which the London skull resembles and those in which it differs from the female skulls of Aurignacian and Solutrean date found in Europe and the female skulls relating to the upper palaeolithic phase in England have already been indicated and discussed. As the cranial fragment in some of its features appeared to approximate to the modern cranial type, it seemed not improbable that some further light might be thrown on its affinities by a comparison of its characters, in so far as they could be determined by measurement, with those of a relatively homogeneous series of modern skulls. This procedure will at least permit due consideration being given to the degree of variation that is normally shown in the several cranial characters in such a series. Whenever possible a comparison of this nature seems to be very desirable, if not essential, before appraising finally the status of a solitary, and possibly incomplete, skull that may be discovered, because of the definite tendency evinced by craniologists to assume that such a find may be considered a more or less average specimen of the population it represents. While, as a rule, the odds are greatly in favour of such an assumption being true, the possibility that the new discovery may be a normal, yet rather extreme, variant and not a truly representative or average specimen of a particular cranial type should always be borne in mind.

For comparison with the London skull, a suitable series of modern female skulls is fortunately available. This series forms part of the large collection of modern skulls, exceeding a thousand in number, which is preserved in the Anatomical museums at the University and in St Mungo's College, Glasgow, fully 700 being at the University and 300 at the College. Of the total series, approximately 400 are considered to be of the female sex. These female skulls exhibit a very close resemblance to those of the same sex in the collection of seventeenth-century Londoners from Whitechapel, the characters of which were described by Macdonell in 1904 in Vol. 3 of *Biometrika*. The female skulls in the Farringdon Street collection of contemporary Londoners described by Miss Hooke in 1926 in Vol. 18 of *Biometrika* are closely similar in type to those found in Whitechapel. The rather striking degree of similarity in the cranial series from Glasgow and London is described and discussed in a paper by the writer entitled "The West Scottish Skull and its Affinities".* Prof. Bryce readily granted permission for some further observations to be made on the female skulls included in the collection.

Reference has already been made to the fact that in the London skull the occipital bone presents the peculiarity that its supra-occipital segment had been divided at one time into two distinct parts, an upper and a lower, by a transverse suture which had become synostosed. This suture was situated some distance above the biasterionic diameter, so, in other words, a pre-interparietal bone had

* *Biometrika*, 1931, Vol. 23, pp. 10–22.

been present. Emphasis is laid on the presence of this anomaly in the skull, as it would appear to have a definite influence in determining the sagittal extent and also the curvature of the occipital region.

In Fig. 4 is shown the superimposition of the median sagittal contour tracing of the London skull on the corresponding contours of two relatively low-vaulted members of the Scottish female series, one (S 73) with, the other (C 45) without, an interparietal bone. The skulls are oriented in the subcerebral plane, the asterions being made coincident. The contour of the skull with the interparietal present seems to be in somewhat closer agreement with the London contour than the other.

Some years ago, during the systematic examination and measurement of the arcs and chords of the bones of the cranial vault in the long series of Scottish skulls, it was noticed that the presence of an interparietal in a skull was usually associated with an occipital arc and an occipital chord which were definitely above the average dimensions, just as an increase in average minimum frontal breadth characterized the skulls in which there was a persistence of the metopic suture.

As it was known that in the Scottish series of females there were included ten or twelve skulls in which an interparietal or a pre-interparietal bone was or had been present, it seemed to be advisable to compare the available measurements of the London skull with the average values for the "interparietal" group, as well as with the averages for a larger series in which there was no evidence that an interparietal had ever been present. The female skulls that exhibited a pre-interparietal or an interparietal, the serial numbers of which were known, were thus first extracted from the collection and set apart as one group. From the remainder of the series, comprising nearly 400 skulls, a random group of about fifty skulls was segregated by extracting specimens at more or less regular intervals throughout its extent.

After orientation in the Frankfort plane, accurate tracings were made by the dioptograph of the left lateral aspect of each of the skulls, including the profile or median contour from the bregma to the opisthion. On these tracings certain points and sutures were indicated, viz. the lambda, the inion, the left asterion, the left porion, and the left suborbital point, as well as the fronto-malar, the coronal, the spheno-parietal, the squamous and the parieto-mastoid sutures. The porion with the suborbital point, and the asterion with the point at the outer end of the sutural junction of the external angular process of the frontal bone with the malar bone provide the data necessary for the insertion of the lines representing the Frankfort plane and the subcerebral plane of Keith, respectively. The various chords were then drawn on the outlines and several measurements were taken corresponding to the measurements that were determinable on the projection of the median sagittal contour of the London skull fragment. The arcs and chords of the total parietal and occipital sagittal sections, together with those of the

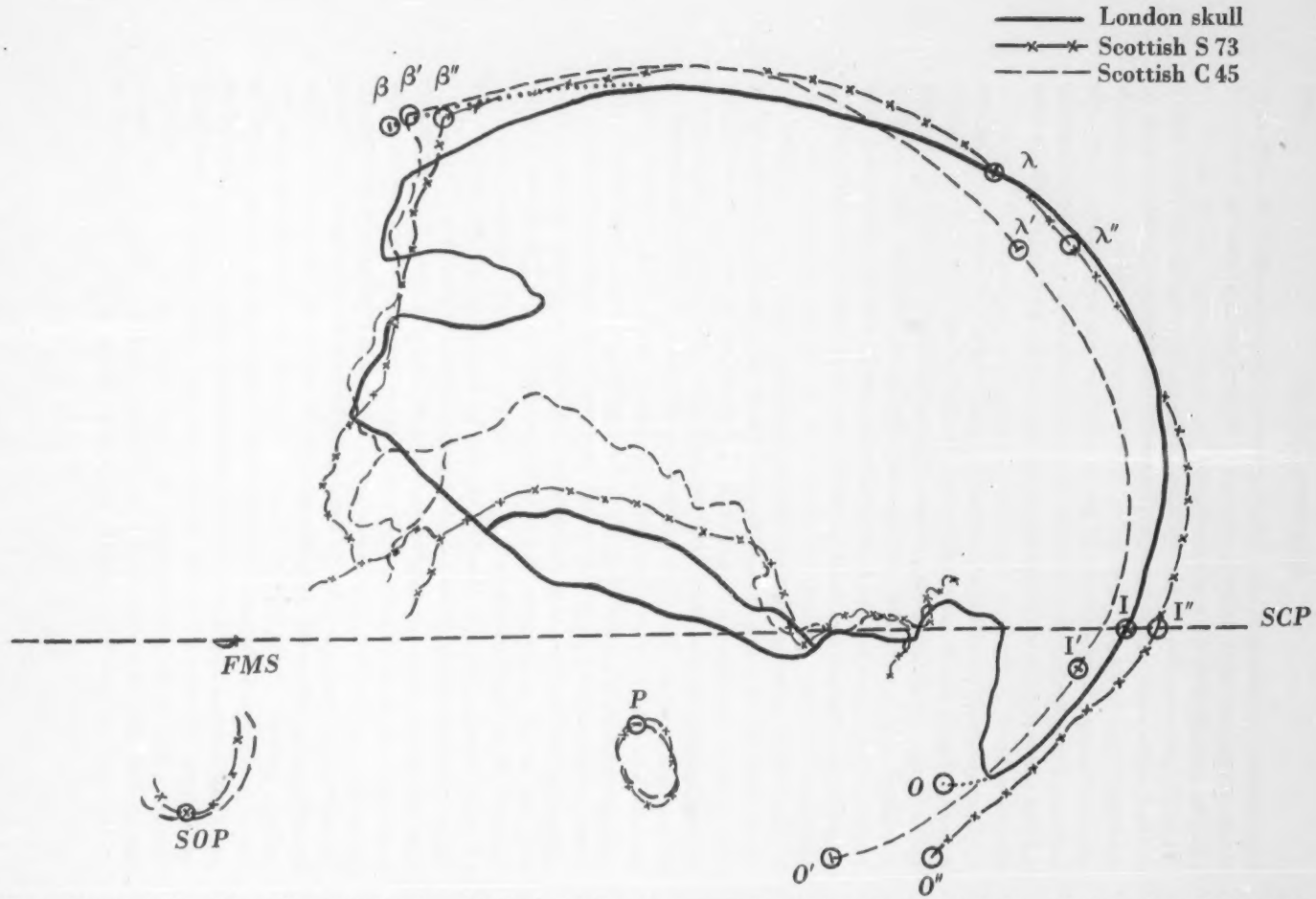

Fig. 4. Superimposition of the median sagittal tracing of the London skull on the corresponding tracings of two relatively low-vaulted members of the Scottish series, one (S 73) with an interparietal present, the other (C 45) with no interparietal. The skulls are oriented in the subcerebral plane, the asterions being made coincident.

supra-inial and sub-inial segments of the occipital section, were also measured directly with the steel-tape and callipers.

In addition to these new direct measurements and those which could be obtained from the incomplete orthographic projections, the measurements of various other cranial characters—including the maximum length, the maximum breadth, the biasterionic breadth and the cubic capacity—were available from previous records for each of the skulls comprising the two groups of the modern Scottish series. The values of the several cranial characters that it was possible to measure on the profile outline of the London skull fragment, and the dimensions of other characters which could be estimated with such a fair measure of probability that they may be supposed reasonable approximations to the true values, are shown in Table II. In the same table are given the mean values and standard deviations computed for a random group of forty-seven normal modern Scottish female skulls, and the same constants for ten specimens from the same series having interparietals or pre-interparietals. In the penultimate column all means of the Farringdon Street series* that are available for the characters used are given. This series has been chosen in preference to that from Whitechapel mainly because the mean values of certain characters relating to the occipital section are only available for the Farringdon Street skulls.

The measurements of most of the characters considered taken on a cast of the London skull and published by H. J. Friederichs† in his memoir on the specimen are also entered in the table, as in some cases they appear to differ greatly from those recorded by observers in this country who have studied the actual bones.

Before the measurements of the London skull fragment are compared in detail with the mean measurements of the two modern female series, attention may be drawn to differences in the two Scottish groups which appear to be associated with the presence of an interparietal. The figures in Table II show that the occipital arc is significantly longer and more curved and the occipital chord significantly longer, on the average, in the group with interparietals present than in the normal group. The average differences in length of arc and chord are 9 and 6 mm., respectively. The occipital curvature may be measured by the crude index, 100 × occipital chord/occipital arc, or by Pearson's occipital index ($Oc.\ I = 100 \times \frac{S_3}{S'_3}\sqrt{\frac{S_3}{24\,(S_3 - S'_3)}}$, where S_3 = occipital arc and S'_3 = occipital chord). The latter index is preferable to the former; it measures the convexity of the occipital bone from the lambda to the opisthion, giving the ratio of the radius of curvature of the bone (supposing the curvature to be that of a circle, which is only roughly the case) to the occipital chord. As the radius of curvature shortens with greater convexity the index obviously decreases correspondingly.

* Taken from *Biometrika*, 1926, Vol. 18, p. 28.

† "Die morphologische Einreihung des 1925 in London City gefundenen paläolithischen Schädels." *Zeit. für Anat. und Entwick.* 1932, 98. Band, pp. 475–486.

series, one (S 73) with an interparietal present, the other (C 45) with no interparietal. The skulls are oriented in the subcerebral plane, the asterions being made coincident.

TABLE II

Showing a Comparison of the Measurements of the London Skull (a) with those of a cast of it given by Friederichs (b), with a group of 47 Scottish female skulls without interparietals (c), with a group of 10 female skulls with interparietals belonging to the same series (e), and with a seventeenth-century London series (g)

	The London Skull		A random group of 47 normal Scottish female skulls		A group of 10 Scottish female skulls with interparietals		A series of seventeenth-century London skulls from Farringdon Street	$\frac{a-c}{d}$‡
	Young	Friederichs	Mean ± S.E.	σ	Mean ± S.E.	σ	Mean	
	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
Maximum length (*L*)	180*	—	178·4 ± 0·74	5·07	179·8 ± 0·99	3·12	181·6 (182)	—
Maximum breadth (*B*)	144?	152 (76 × 2)	135·5 ± 0·68	4·66	135·4 ± 1·77	5·58	135·7 (180)	1·8
Basio-bregmatic height (*H'*)	120*	131·0	126·7 ± 0·64	4·40	128·7	—	122·5 (163)	—
Auricular height (*OH*)	—	114·0	107·3 ± 0·47	3·20	107·8	—	105·2 (69)	—
Height of cranial vault above subcerebral plane	90	—	98·2 ± 0·67	4·60	98·6 ± 1·10	3·47	—	−1·8
Height of squamous suture above subcerebral plane	22	—	30·5 ± 0·65	4·43	27·5 ± 1·44	4·55	—	−1·9
Biasterionic breadth (*B'''*)	114	116·0	106·2 ± 0·60 (40)	3·81	107·0 (5)	—	—	2·1
Parietal arc (S_2)	105	130·0	122·7 ± 0·91	6·25	117·1 ± 1·83	5·80	123·8 (201)	−2·8
Occipital arc (S_3)	123	128·0	114·5 ± 0·96	6·59	123·5 ± 1·93	6·10	114·8 (182)	1·3
Lambda-inion arc ($S_{3\cdot1}$)	82	81·5	60·9 ± 1·13	7·77	69·5 ± 2·06	6·50	[68·0]† (64)	2·7
Inion-opisthion arc ($S_{3\cdot2}$)	41	46·5	53·7 ± 0·76	5·18	54·0 ± 1·81	5·74	[46·0] (64)	−2·5
Parietal chord (S'_2)	101	121·0	109·9 ± 0·69	4·71	106·3 ± 1·37	4·35	110·3 (201)	−1·9
Occipital chord (S'_3)	100	104·0	94·8 ± 0·70	4·78	100·6 ± 1·39	4·40	94·0 (184)	1·1
Lambda-inion chord ($S'_{3\cdot1}$)	76	74·0	57·4 ± 0·97	6·62	64·6 ± 1·75	5·52	[63·0] (64)	2·8
Inion-opisthion chord ($S'_{3\cdot2}$)	40	46·3	52·3 ± 0·73	5·04	52·4 ± 1·61	5·10	[45·0] (64)	−2·4
Height of parietal arc	12	21·5	22·7 ± 0·34	2·32	20·7 ± 0·84	2·67	[23·0]	−4·6
Height of lambda-inion arc	15	15·0	8·2 ± 0·32	2·21	10·2 ± 0·77	2·44	[10·0]	−3·1
Length of parieto-occipital segment of skull parallel to subcerebral plane	130	—	129·1 ± 0·76	5·19	130·3 ± 1·12	3·53	—	0·2
Lambda-inion-opisthion angle	117°	116°·0	121°·3 ± 0·56	3°·84	120°·1 ± 1·08	3·40	[120°·0]	−1·1
Lambda-inion subcerebral plane angle	77°	—	81°·9 ± 0·53 (30)	2°·91	81°·4 ± 1·14 (9)	3°·43	—	−1·7
Opisthion-inion subcerebral plane angle	40°	—	39°·9 ± 0·59 (30)	3°·25	39°·8 ± 1·01	3°·03	—	0·0
Length-breadth (cephalic) index (100 *B*/*L*)	80*	—	76·0	—	75·2	—	74·8 (167)	—
Length-height (altitudinal) index (100 *H'*/*L*)	—	—	71·0	—	71·6	—	67·9 (155)	—
Height-breadth index (100 *B*/*H'*)	—	—	106·9	—	105·2	—	110·6 (158)	—
100 × cerebral height/basio-bregmatic height	—	—	77·5 ± 0·49	3·38	76·7	—	—	—
100 × cerebral height/auricular height	—	—	91·6 ± 0·54	3·70	91·7	—	—	—
Index of parietal curvature (100 S'_2/S_2)	96·2	93·8	89·8 ± 0·25	1·75	90·8 ± 0·40	1·26	89·1 (201)	3·7
Index of occipital curvature (100 S'_3/S_3)	81·3	81·2	82·9 ± 0·26	1·80	81·5 ± 0·58	1·84	81·9 (182)	−0·9

TABLE II (*continued*)

	The London Skull		A random group of 47 normal Scottish female skulls		A group of 10 Scottish female skulls with interparietals		A series of seventeenth-century London skulls from Farringdon Street	$\frac{a-e}{d}$ ‡
	Young	Friederichs	Mean ± S.E.	σ	Mean ± S.E.	σ	Mean	
	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
Pearson's occipital index	58·1	—	59·7 ± 0·25	1·70	58·4 ± 0·49	1·57	59·1 (181)	−1·0
Index of curvature of supra-inial arc ($100 \times S'_{3\cdot1}/S_{3\cdot1}$)	92·7	90·8	94·5 ± 0·34	2·31	93·0 ± 0·64	2·02	92·6 (64)	−0·8§
Index of curvature of infra-inial arc ($100 \times S'_{3\cdot2}/S_{3\cdot2}$)	97·6	99·6	97·5 ± 0·25	1·69	97·1 ± 0·53	1·66	97·8 (64)	0·1§
100 × parieto-occipital segmental length/maximum length (L)	—	—	72·4 ± 0·33	2·29	72·5 ± 0·73	2·30	—	—
100 × lambda-inion arc/occipital arc (S_3)	66·7	63·7	53·0 ± 0·72	4·92	56·3 ± 1·33	4·22	59·2	2·8
100 × inion-opisthion arc/lambda-inion arc	50·0	57·1	90·2 ± 2·54	17·4	78·7 ± 4·43	14·0	67·6	−2·3
Parietal arc height index	11·9	17·8	20·6 ± 0·24	1·68	19·4 ± 0·55	1·72	21·3	−5·2
Lambda-inion arc height index	19·7	14·4	14·1 ± 0·38	2·61	15·6 ± 0·82	2·59	15·9	2·2
100 × biasterionic B/parietal breadth (B)	79·2	—	78·3 ± 0·44 (40)	2·84	78·3 (5)	—	—	0·3
100 × biasterionic B/parietal chord (S'_2)	112·9	—	96·5 ± 0·80 (40)	5·05	101·2 (5)	—	—	3·3
100 × biasterionic B/occipital chord (S'_3)	114·0	111·5	111·6 ± 1·01 (40)	6·41	105·1 (5)	—	—	0·4

* Measurements of the London Skull followed by an asterisk are merely estimated values.

† Measurements of the Farringdon Street skulls given in square brackets [ ] are estimated from the mean sagittal contour tracings published in *Biometrika*, 1926, Vol. 18.

‡ It is important to emphasize that for all the characters, with two unimportant exceptions, in which the ratios in the last column of the table exceed 2, the mean values of the Scottish group with interparietals are less divergent from the corresponding measurements in the London skull than are the mean values of the Scottish group without this anomaly.

§ It is apparent in these cases that the distributions in the Scottish series are not approximately normal, and hence that the ratios are criteria of a different kind from the others.

Since it may be suggested that forty-seven skulls is a small number on which to base a standard of normality, it may be mentioned that the same striking differences are apparent on comparing the mean values of the characters under consideration in the interparietal group with the corresponding mean values derived from the whole series of female skulls, fully 370 in number. Closely analogous differences are shown, moreover, in comparing the mean values of the same three characters in the group of twelve male skulls showing interparietals or pre-interparietals with the corresponding mean values in the total series of fully 500 male skulls, though it should be stated that the difference in the mean occipital index in the male groups cannot be regarded as certainly significant as it is not quite twice its standard error.

On comparing the supra-inial and sub-inial segments of the occipital arc in the two female groups, it is seen that, as might be expected, the divergence which has been referred to is restricted to the upper region. The lambda-inion arc is

approximately 9 mm. longer and the lambda-inion chord approximately 7 mm. longer on the average in the group with interparietals than in the normal group. These excesses are statistically significant. The difference in the index of curvature (100 × chord/arc) of the supra-inial segment in the two groups exceeds twice its standard error, indicating that the arc in the interparietal group may be regarded as sensibly more convex than in the normal group. In the sub-inial region, however, the respective measurements of the mean length and curvature of the arc and the mean length of the chord are practically identical in the two groups.

The parietal arc and chord and index of curvature (100 × chord/arc) also show differences between the two series. The absolute measurements are both sensibly greater and the ratio sensibly less on the average in the normal group than in the group with interparietals. As the upper occipital arc is, on the average, definitely longer in the anomalous than in the normal group, there would appear to be present a tendency to an inverse relationship in the lengths of the supra-occipital and parietal arcs. In the group of fifty-seven modern skulls, the correlation between the lengths of the parietal and lambda-inion arcs is $-0{\cdot}227 \pm 0{\cdot}126$.* Though the existence of a slight tendency to an inverse relationship in the extent of the arcs seems to be suggested by the coefficient, in view of the size of the standard error it cannot be said to be definitely significant for the measurements available. In the total group of 376 West Scottish female skulls there is, however, a small but statistically significant inverse association between the lengths of the total occipital and the parietal arcs, the coefficient being $-0{\cdot}182 \pm 0{\cdot}050$, a long occipital arc showing a slight tendency to be associated with a short parietal arc and vice versa. In the long Egyptian series of female skulls of the 26th to 30th dynasties, the compensatory relationship in the two corresponding cranial arcs is more emphasised than in the Scottish series, the coefficient of correlation between the lengths of these two segments of the sagittal arc being $-0{\cdot}342 \pm 0{\cdot}037$.†

The definite differences observed in the mean measurements for the two groups of skulls from the same relatively homogeneous Scottish series—that with and that without interparietals—suggest strongly that the relatively long occipital bone and the relatively short parietal bone, which are features of the London skull fragment that have been noted by various observers, are really largely dependent on the fact that it has a pre-interparietal bone.

We pass now to a comparison of the measurements of the characters in the London skull fragment with the corresponding mean measurements in the modern Scottish female series and in the seventeenth-century series of female Londoners. A brief scrutiny of the figures in Table II shows the close correspondence that obtains in general between the corresponding average values in these two series. The comparison will be mainly concerned with the mean values

* The symbol ± denotes standard errors throughout this paper.

† *Biometrika*, 1924, Vol. 16, p. 361.

in the Scottish series, and only when these differ appreciably from the values in the London series will particular reference be made to the latter. The deviation of the measurement of any character in the London skull from the mean value of the corresponding character in the normal Scottish series was divided by the standard deviation of the character for this series. The ratios thus obtained vary considerably in value. When the ratio is 1·0, then 1 skull in 6 has the characteristic more emphasized in the given direction than it is in the London skull; if 1·5 then 1 skull in 15; if 2·0 then only 1 in 45; if 2·3 then only 1 in 93; if 2·5 only 1 skull in 160. In statistical judgments, a characteristic is not usually regarded as individually remarkable unless it exhibits a difference *in the given direction* that is not equalled or exceeded more than once in about 45 times or more, that is, when the ratio equals at least 2.

Applying this test to the various characters of the London skull and beginning with the occipital region, we find, as shown in the last column of Table II, that though the measurements of the occipital (lambda-opisthion) arc, the occipital chord and the occipital indices of curvature—both the crude one ($100 \times$ chord/arc) and Pearson's—in the specimen deviate to some extent from the mean values in the normal Scottish female series, yet the divergences are not so great that the characters can be considered exceptional for this series. The supra-inial arc curvature and infra-inial arc curvature in the cranial fragment are also of such a degree as might be likely to occur not infrequently in the normal Scottish female, but the supra-inial arc and chord are more extensive, and the infra-inial arc and chord less extensive than might reasonably be expected to appear except rarely in this modern type. Agreement with the Scottish skull type in these respects is definitely closer, however, when interparietals are present, as is shown by the fact that the lengths of the occipital arc and the occipital chord, and the curvatures of the total occipital and supra-inial arcs in the London specimen, practically coincide with the corresponding mean values in the anomalous or interparietal Scottish group. Dimensions of the supra-inial arc and chord corresponding in extent to those found in the London skull might also be expected to occur with greater frequency in the Scottish group with interparietals than in the normal group.

In view of the relatively small extent of the sub-inial region of the London skull in the sagittal plane, it should be mentioned that some allowance has been made in the estimated measurements of the occipital bone for the small area that has been detached and lost at the opisthion. The disproportion in length of the supra-inial and sub-inial segments of the occipital arc in the London skull is very striking. In this specimen, as will be seen from reference to Table II, the ratio of the inion-opisthion arc to the lambda-inion arc is 50 per cent., as compared with corresponding values of approximately 90 and 70 per cent. in the Scottish and Farringdon Street series, respectively. A possible explanation of this feature may be provided from analogy with a relationship which is found in the Scottish

skull. In the Scottish series of fifty-seven skulls there is a very definite inverse association between the lengths of the supra-inial and sub-inial arcs, as is shown by a coefficient of correlation of $-0{\cdot}454 \pm 0{\cdot}105$, a long supra-inial segment tending to be associated with a relatively short infra-inial segment.

The London skull is also incomplete in the region of the bregma, but the approximate position of this anthropometric point or landmark can be indicated by continuing the outline tracing forward till it meets the line of the coronal suture. The parietal arc measured from the lambda to the bregma determined in this way is characteristically less than the corresponding measurement in the normal Scottish series (which is identical with that in the recent Londoners). The parietal curvature, as measured by the index, $100 \times$ parietal chord/parietal arc, is also characteristically less, and the parietal chord in the London skull is of a length which might reasonably be expected to occur occasionally in the modern female. From analogy with the definite tendency to an inverse relationship in occipital and parietal arc lengths, which has been shown to exist in both the Scottish and Egyptian female skulls, the occurrence of a relatively short parietal arc in the London skull might almost be expected, as its occipital arc is relatively long and the supra-inial segment of that arc exceptionally long.

It should be mentioned here that, while the measurements cited for the lengths of the sagittal parietal arc and chord are almost identical with those estimated from the profile drawing of the London skull given by Sir Arthur Keith,* they differ greatly from those for the corresponding characters given by Friederichs in his memoir on the specimen. This observer estimates the parietal arc length to be 130 mm. and the chord 121 mm. Reasoned consideration of the probable form and dimensions of the skull based on the fragment that is preserved has convinced the writer that measurements of this order for the parietal bone are obviously beyond the range of what is not merely probable but possible. The same criticism seems to be applicable to some of the estimates of measurements of other principal characters of the London skull recorded by Friederichs and cited in Table II. Half the maximum breadth is tabulated by him as 76 mm., which multiplied by 2 would give a maximum width of 152 mm., though in a later table the width is given as 144–146 mm. His estimates of the basio-bregmatic height and auricular height as 131 and 114 mm., respectively, appear to be much in excess of the most probable values.

Certain other characters in the London skull may now be compared with the corresponding characters in the modern series. A notable feature of the cranial fragment on which much emphasis has been laid is the apparent lowness of the vault. The highest point of the vault—i.e. the vertex—lies according to Sir Arthur Keith only 90 mm., approximately, above the level of the subcerebral or basal plane. The average height of the summit of the vault above the same base line in the Scottish random series of forty-seven female skulls is 98·2 mm. As

* *New Discoveries relating to the Antiquity of Man*, 1931, p. 446 *et seq.*

the same height of the London skull does not deviate from this average value by twice the standard deviation of the elevation in the Scottish series it cannot be considered a value so low that it is unlikely to appear occasionally in this series; indeed, in the group of fifty-seven skulls (forty-seven normal and ten anomalous) three specimens are found in which the subcerebral heights of the vault are 88, 89 and 90 mm., respectively, while seven other members of the series do not exceed 93 mm. in altitude. The height of the cranial vault above the subcerebral plane in the London skull cannot, therefore, be described as extremely low in comparison with that found in the modern type.

In the sample of Scottish female skulls, the highest point on the squamous suture lies on the average 30·5 mm. above the level of the subcerebral plane. In the London fragment the corresponding height is 22 mm. As the standard deviation in the Scottish series is 4·4 mm. the height of the suture in the London fragment cannot be considered a value which would be exceptional for the Scottish series; indeed, the actual elevation of the suture above the basal plane in this series varies from 18 to 40 mm. Sir Arthur Keith notes as a point of contrast between the London skull and a modern "river-bed" type of skull with which he compared it, the greater rapidity with which in the former the lower or squamous border of the parietal rises as it passes forward. A brief survey of the outline tracings of the modern Scottish series supplies ample evidence that this feature is extremely variable even in such a homogeneous group, and that a wide range in degree of inclination is found; in some cases the direction of the suture in its posterior part almost approximates to the vertical.

From a comparison of a low-vaulted skull from the "river-bed" series with the London skull by superimposition of the profile outlines, Sir Arthur Keith came to the conclusion that one of the features in which the London skull resembles the skull from Piltdown, but differs from specimens of the modern type, was that in the first-named specimen the sub-inial or nuchal part of the skull descended in a more vertical direction. In reaching this conclusion, he does not appear, however, to have made adequate allowance for the normal range of variation in this feature in the modern skull. A rough estimate of the degree of flexion of the lower sub-inial segment of the occipital bone on the supra-inial segment may be obtained by measuring the lambda-inion-opisthion angle, i.e. the angle enclosed by the chords of the two segments of the occipital bone. The size of this angle is not a very reliable index of the degree of flexion, however, as it is influenced to such an extent by the variable position of the inion. In the Scottish series it ranges in value from 111 to 127° with an average of 121°. In the London skull the angle is 117°, which is well within the limiting values of the modern group.

The acuteness of the forward flexion of the sub-inial segment may also be estimated in some measure by the size of the angle between the inion-opisthion chord and the line denoting the subcerebral plane. This angle in the London skull

is 40°. In this skull, the internal occipital protuberance, unlike the arrangement in the Neanderthal type, coincides in level with the inion. In the thirty specimens from the Scottish female series in which the subcerebral plane passes approximately through the inion the average value of the angle is 39°·9 and the range of variation from 33 to 45°. These comparisons seem to indicate that in regard to the curvature of the lower occipital region, the London skull cannot be held to differ in a significant degree from the specimens of modern type.

It has already been mentioned that one of the main differences between the fragment of the London skull which has been preserved and the corresponding part of the Neanderthal type of skull is seen in the lower part of the occipital region. In the latter type the sub-inial part of the occipital bone does not continue downward the line of curvature of the upper segment but is bent somewhat abruptly forwards at the inion corresponding to the flattened form of the cerebellum.

On account of the defect in the right parietal region of the London skull it is not easy to determine with precision the maximum parietal breadth, but it is probably at least 144 and possibly 146 mm. Sir Arthur Keith gives the measurement as 140 mm., but this would appear to be an under-estimate, as he states that the width of the endocranial cast is 136 mm. and that the thickness of the skull wall varies from 5 to 7 mm. The thickness of the cranial wall is at least 5 mm. at the widest part of the parietal region. In the Scottish series of forty-seven female skulls the average maximum breadth is 135·5 mm. The standard deviation of the breadth in the group is 4·7 mm., so that a cranial width of 144 mm. is a measurement that might reasonably be expected to occur occasionally in such a modern series; indeed, in the series are present two specimens with maximum parietal breadths of 145 and 146 mm., respectively. It is important to note that the maximum parietal breadth of the London skull is found at a point well forward on the parietal bone, and not relatively far back as occurs in skulls of Neanderthaloid type.

A defect in the left asterionic region makes it difficult to estimate with accuracy the biasterionic diameter of the London skull, but it appears to be approximately 114 mm. In forty skulls of the modern Scottish female series the mean biasterionic breadth is only 106·2 mm. An individual measurement as great as 114 mm. in this series must be regarded as rather exceptional. The proportions which the biasterionic diameter in the London skull bears to the maximum parietal breadth (B) and to the length of the occipital chord (S'_3), respectively, are, however, in fairly close agreement with the corresponding average indices in the modern Scottish female series.

Having considered the characters of the London skull fragment that can be measured with a reasonable approach to accuracy, it may be of interest to place on record some observations on the probable length, form and cubic capacity of the cranium when complete. An estimate of the original maximum length may

be obtained from the different dimensions of the fragment that are available. Prof. Karl Pearson* found in his very long series of Egyptian female skulls that of the three segments of the sagittal arc—frontal, parietal and occipital—the last (S_3) shows the highest correlation with the maximum length (L) and would give the most accurate prediction of this character. In the Scottish female series of 375 skulls, the correlation between the maximum length (L) and the length of the occipital arc (S_3) is $0{\cdot}501 \pm 0{\cdot}039$, which is of much the same order as the value ($r = 0{\cdot}442 \pm 0{\cdot}034$) found in the long Egyptian female series. The linear regression equation expressing maximum length in terms of occipital arc length in the Scottish series is

$$L = 0{\cdot}38S_3 + 136{\cdot}00,$$

with a standard error of prediction of 4·5 mm. If this equation be assumed to be applicable to the London skull, in which the length of the occipital arc equals 123 mm., the maximum length may be considered to be approximately 183 mm.

The length of the parieto-occipital segment of the London skull from the occipital contour line to the approximate position of the coronal suture, near the point where this suture is crossed by the lower temporal line, measured in a direction parallel to the subcerebral plane, is 130 mm. The correlation coefficient between the length of this parieto-occipital segment ($P.O.L.$) and the glabella-occipital length (L) in the Scottish series of fifty-seven female skulls is $0{\cdot}543 \pm 0{\cdot}093$. The regression equation expressing maximum length in terms of parieto-occipital length is

$$L = 0{\cdot}52P.O.L. + 111{\cdot}14,$$

with a standard error of prediction of 4 mm. Assuming this equation to be applicable to the London skull, in which the parieto-occipital segment is 130 mm., the predicted maximum length would be 179·3 mm. or approximately 180 mm. This estimate of length is probably nearer the true value than that based on the regression formula for the length of the occipital arc. The maximum cranial length may be estimated in a simpler way. In the Scottish female series the average length of the parieto-occipital segment is 129 mm., i.e. almost identical with the corresponding measurement in the London skull. The length of this segment expressed as a proportion of the maximum length has in the series an average value of 72·4 per cent. with a range of variation from 67 to 77 per cent. If we assume that the parieto-occipital segment in the London skull is 72·4 per cent. of its maximum length, the maximum length of the skull would again be 179·6 mm. On the assumption that the parieto-occipital length just described is on the average approximately 70 per cent. of the total length in modern skulls, Sir Arthur Keith suggests that the probable length of the London skull was 185 mm. On the assumption that the mean ratio in the Scottish modern female series may be considered to represent approximately the relationship in the

* *Biometrika*, 1924, Vol. 16, Table V, facing p. 348.

London skull, the maximum length in the latter may be held to be most probably about or just under 180 mm., but it should be borne in mind that the actual length may be appreciably greater or less than this estimate. Let us consider for a moment the limiting values found for the ratio 100 × parieto-occipital length/greatest length, in the homogeneous Scottish group. A value of 67 per cent. for the ratio would correspond to a total length of 194 mm., while a ratio of 77 per cent. would postulate a length of 169 mm. Though, as is known from the general form and extent of the fragment preserved, neither of these limiting lengths is probable, indeed possible, for the London skull, yet they serve to indicate that the most probable length of 180 mm. may be appreciably in excess or defect of the true value.

A maximum length of 185 mm. and a maximum breadth of 140 mm., as estimated by Sir Arthur Keith, give a cephalic index of 75·7, which practically coincides with the mean value for the complete Scottish female series. A maximum length of 180 mm. and a maximum breadth of 144 or 146 mm. give a cephalic index of 80 or 81, but even such a relatively high index cannot be considered one that is unlikely to be found occasionally in the Scottish female skull.

Having considered the probable maximum length of the skull, we may refer briefly to its probable cubic capacity. Various formulae have been computed—notably by Pearson and Lee* and Hooke†—for the purpose of determining the cranial capacity from the absolute linear measurements of the skull length, breadth and height (auricular or basio-bregmatic) or their product ($L \times B \times H'$ or $L \times B \times OH$). As it is possible that in other fragmentary skulls, like that found in London, the height of the vault from the subcerebral plane may be determinable when neither basio-bregmatic nor auricular height can be ascertained, the linear regression equation expressing the cranial capacity in terms of the product of the absolute measurements of length, breadth and subcerebral height has been calculated, based on the data which are available for the fifty-seven Scottish female skulls. In this series the correlation between the subcerebral height (H'') and the cubic capacity (C) is almost as high as that between the maximum length and the cubic capacity, the respective coefficients being $0{\cdot}504 \pm 0{\cdot}099$ and $0{\cdot}529 \pm 0{\cdot}095$. The equation is as follows:

$$C \text{ (in c.c.)} = 0{\cdot}000465 \times (L \times B \times H'') + 228{\cdot}84.$$

The standard error of prediction is 60 c.c. This formula may be applied to estimate the cubic capacity of the London skull. Using the values of the three linear dimensions which have been suggested as most probable as a result of the present study—viz. length (L) = 180 mm., breadth (B) = 144 mm. and subcerebral height (H'') = 90 mm.—the estimated capacity is 1314 c.c. From the

* *Phil. Trans.* 1899, Vol. 196 A, pp. 225–264.

† *Biometrika*, 1926, Vol. 18, pp. 33 and 34.

linear dimensions given by Sir Arthur Keith—viz. length = 185 mm., breadth = 140 mm. and subcerebral height = 90 mm.—the predicted cubic capacity is exactly the same. This estimate is rather greater than the cubic content, viz. 1260 c.c., cited by this author as determined by the application of the formula of Pearson and Lee, but the difference does not exceed the standard error of prediction in using the equation which is given. If we may consider the cubic capacity as being probably in the region of 1300–1320 c.c. we have an estimate which does not diverge appreciably from the mean cubic capacity for 376 female skulls in the Scottish series, viz. $1329 \pm 3{\cdot}6$ c.c.

Though it must be admitted that the prediction of the maximum length of the skull from the antero-posterior extent of its parieto-occipital segment can only be considered a rough approximation to the true value, the cubic content computed from the product of the length thus estimated and the other two linear dimensions that can be estimated with a greater degree of accuracy suggests that it does not diverge greatly from the average cranial capacity found in a group of modern female skulls of a type closely related to that found in seventeenth-century Londoners.

A survey of the mean values of the several cranial characters in Table II shows that one feature in which the seventeenth-century London female differs from the modern Scottish female and approaches more nearly to the condition found in the London skull fragment is the relative proportion of the two segments of the occipital bone. The lengths of the complete occipital arc and chord and the degree of curvature of the occipital arc as a whole in the recent Londoner are almost identical with the corresponding characters in the normal Scottish series. The lengths of the supra-inial segment of the occipital arc and the supra-inial chord and the degree of curvature of the supra-inial segment in the seventeenth-century Londoner are, on the other hand, in close agreement with the corresponding characters in the Scottish group with interparietals, and like these sensibly divergent from the corresponding values in the normal group, whereas the infra-inial arc and chord in the seventeenth-century Londoner are definitely shorter than the corresponding characters in either of the Scottish groups and lie nearer to the estimated values of the London skull than to these means.* The difference in the relative proportions of the two segments of the occipital arc in the two groups is shown clearly by the tabulated values of either of the two ratios: $100 \times$ inion-opisthion arc/lambda-inion arc and $100 \times$ lambda-inion arc/occipital arc.

Some of the main points brought to light from a study of the figures given in Table II to which reference has been made in the preceding section of the text may be summarized briefly here. The standard deviations of the several charac-

* It is possible that the disagreement between the Scottish and London series in the measurements taken from the inion may be due partly to differences in the way in which this point was located by the different observers.

ters in the Scottish series may be accepted as rough measures of the variabilities that might be expected in the corresponding characters in the London skull were it a member of a series, and give some indication of the error that may be made in assuming the specimen to be an average representative of its type. From the differences that are observed in the mean measurements of the two Scottish female groups—the normal and that with interparietals—there is reasonable ground for the inference that the presence of this anomalous bone in the London skull has probably had a definite influence in determining the rather unusual extent and curvature of its occipital region. Of the differences that are observed in the measurements of the characters in the London skull and the corresponding mean values of the Scottish series, only a few are of such a magnitude as to indicate that the measurements in the specimen may reasonably be considered exceptional for the Scottish sample. Amongst these, the principal are the relatively long supra-inial and short infra-inial segments of the occipital arc and the relatively short and flat parietal arc. It is not improbable, however, that in the London skull, the short parietal arc, as well as the relatively short infra-inial arc, may be to some extent compensatory to the long supra-inial arc which is associated with the presence of the pre-interparietal.

In height of cranial vault above the subcerebral plane, the London skull cannot be said to be exceptionally low for a modern skull of the Scottish type, and in fullness of curvature of the lower occipital region it appears to be not greatly divergent from, if not in close agreement with, the latter.

Comment is made on the estimates of the dimensions of some of the characters cited from Friederichs' memoir on the London skull. His measurements were taken on a cast. Those given for the lengths of the parietal arc and chord and the basio-bregmatic height, at least, appear from the form and dimensions of the actual fragment of skull that is preserved to be very improbable, if not impossible, approximations to the true values.

(*d*) *Comparison with the Neanderthal Female Skulls from Gibraltar, La Quina and Saccopastore* (*Rome*). In view of the alleged Neanderthaloid features in the London skull fragment, to which reference has already been made, and its alleged affinities with the Piltdown skull, which have been discussed by Sir Arthur Keith,* it seemed to be of interest to compare in detail with the measurable characters of the London skull the measurements of the corresponding characters in the three skulls of Neanderthal type which are generally admitted to be of the female sex, namely the Gibraltar I, the La Quina (adult) and the Saccopastore, and also those in the Piltdown skull. A brief account will also be given of the corresponding characters in the recently discovered Swanscombe skull.

Unfortunately, on account of deficiencies in the respective crania, some of the characters that have been under review cannot be measured in such a manner as to warrant any considerable degree of confidence in the probable accuracy of the

* *New Discoveries relating to the Antiquity of Man*, 1931, p. 446.

estimates obtained. Such measurements of characters have been tabulated, however, as may probably be deemed at least fair approximations to the true values, and a few comments will be made on their relationships to the corresponding measurements in the London skull and, incidentally, to those in the modern Scottish series; these measurements are given in Table III.

The measurements of the La Quina and Gibraltar specimens which are given in Table III have been taken from the measurements and contour tracings published by Dr G. M. Morant.* The absolute measurements of the arcs and chords of the Saccopastore skull were very kindly supplied by Prof. Sergio Sergi at the request of the late Sir Grafton Elliot Smith. We are greatly indebted to Prof. Sergi for permission to use these measurements before his final memoir on this very important skull is published. The measurements of the Piltdown skull are taken from the reconstruction made by Prof. Elliot Smith with the assistance of Dr John Beattie. The measurements of the Swanscombe skull were taken from the actual specimen by the courtesy of Mr A. T. Marston.

In preliminary accounts of the Saccopastore skull Sergio Sergi† states that among the other Neanderthal skulls, the specimen from Gibraltar (Gibraltar I) is the one that approaches nearest the Saccopastore in general dimensions and in morphological type. As the Saccopastore skull is not only the specimen most nearly complete but also the one that in its general dimensions is nearest the London skull fragment, the main comparison will be made between the characters of the London skull and this specimen as representing the Neanderthal female type.

In maximum length (L), maximum parietal breadth (B), biasterionic breadth (B''') and cephalic index ($100\ B/L$) the Saccopastore skull is not very different from the London skull fragment. With a basio-bregmatic height of 109 mm., however, the vault of the former is apparently much lower than what may be considered the most probable height for the London skull (*ca.* 120). The relative shortness of the parietal segment (S_2) and relatively great extent of the occipital segment (S_3) of the sagittal arc in the London skull have already been commented upon, and their probable association with the presence of a pre-interparietal bone in this fragment discussed. Although the parieto-occipital segment of the sagittal arc (S_2+S_3) in the London skull is exactly equal to the corresponding segment in the Saccopastore skull, both being 228 mm., there exists a definite disproportion in the length of this segment formed by the parietal and occipital bones in the two specimens. The London skull parietal arc, though relatively short, is 105 mm., but that in the Saccopastore is 86 mm., i.e. 19 mm. less, whereas the occipital arc of the former is only 123 mm. as compared with 142 mm. in the Saccopastore specimen.

* "Studies of Palaeolithic Man. II." *Annals of Eugenics*, 1927, Vol. 2.

† (i) "Le Crâne Néanderthalien de Saccopastore (Rome)." *L'Anthropologie*, 1931, Vol. 41, p. 241. (ii) "Some Comparisons between the Gibraltar and Saccopastore Skulls." *Proc. 1st Internat. Congress Preh. and Protoh. Sciences, London*, 1932, pp. 50–52.

TABLE III

Showing the Measurements of the Available Characters in the London Skull in comparison with the Corresponding Measurements in (1) *the three Neanderthal female Skulls: Gibraltar, La Quina and Saccopastore,* (2) *the Piltdown Skull and* (3) *the Swanscombe Skull*

Characters	Symbols	The London skull	The Gibraltar I skull*	The La Quina skull (adult)*	The Saccopastore skull†	The Piltdown skull‡	The Swanscombe skull§
Maximum length	(L)	180‖	192·5	204·2	181 ?	194‖	180‖
Maximum parietal breadth	(B)	144	149·0	138·3	142·	150	142
Basio-bregmatic height	(H')	—	120·2 (Sergi)	—	109	—	126
Biasterionic breadth	(B''')	114	118·0 ?	112·4 ?	115	123 ?	121
Parietal arc	(S_2)	105	—	107·0	86	120	114
Occipital arc	(S_3)	123	110·0	—	142	125	115
Lambda-inion arc	$(S_{3\cdot1})$	82	65·0	66·3	90	70¶	63
Inion-opisthion arc	$(S_{3\cdot2})$	41	46·0	—	52	55	52
Parietal chord	(S'_2)	101	—	103·0	83	110	106
Occipital chord	(S'_3)	100	85·0	—	103	102	94
Lambda-inion chord	$(S'_{3\cdot1})$	76	60·0	57·8	80	65	58
Inion-opisthion chord	$(S'_{3\cdot2})$	40	44·6	—	50	53	51
Cephalic index	$(100\ B/L)$	80·0‖	77·5	67·7	78·5	78·0‖	79·0‖
Altitudinal index	$(100\ H'/L)$	—	—	—	60·2	—	70·0‖
Index of parietal curvature	$(100\ S'_2/S_2)$	96·2	—	96·3	96·5	91·7	93·0
Index of occipital curvature	$(100\ S'_3/S_3)$	81·3	77·3	—	72·5	81·6	81·7
Pearson's occipital index	$(Oc.\ I.)$	58·1	55·4	—	53·7	58·3	58·4
Index of curvature of supra-inial arc	$(100\times S'_{3\cdot1}/S_{3\cdot1})$	92·7	92·3	87·2	88·9	92·9	92·1
Index of curvature of infra-inial arc	$(100\times S'_{3\cdot2}/S_{3\cdot2})$	97·6	97·0	—	96·2	96·4	98·1
Lambda-inion arc/occipital arc	$(100\ S_{3\cdot1}/S_3)$	66·7	59·1	—	63·4	56·0	54·8
100 Biasterionic B/occipital chord	$(100\ B'''/S'_3)$	114·0	138·8	—	111·7	120·6	128·6
100 Biasterionic B/maximum parietal breadth	$(100\ B'''/B)$	79·2	79·2	81·3 ?	81·0	82·0	85·2
100 Biasterionic B/parietal chord	$(100\ B'''/S'_2)$	112·9	—	109·2 ?	138·6	111·8	114·2
Occipital (lambda-inion-opisthion) angle	—	117°·0	107°·0	—	—	125°·0	114°·0

* Measurements of the Gibraltar and La Quina skulls are taken from a memoir by G. M. Morant, *Annals of Eugenics*, 1927, Vol. 2, pp. 318–381.

† Measurements supplied by Prof. Sergio Sergi.

‡ From the reconstruction by Sir Grafton Elliot Smith and Dr John Beattie.

§ Measurements taken by permission of Mr A. T. Marston.

‖ Measurements indicated thus (‖) are merely estimated values.

¶ Estimated to be 75 mm. by Sir Arthur Keith.

The phenomenally short parietal arc in the Saccopastore skull is undoubtedly associated with the presence in the region of the lambda of several accessory ossicles which, if taken together, would correspond in position and extent with a fair-sized pre-interparietal bone. The point which Sergi has identified as the 'real lambda' and to which he has measured the arcs is L_1, the point at which the interparietal (or sagittal) suture meets the lambdoid ossicle most anteriorly placed. This is also the point in the middle line, or in the sagittal plane, at which the lines that correspond to the directions of the lateral parts of the lambdoid suture meet when prolonged medially. When Wormian bones are found in the region of the upper part of the occipital squama this, as described in Martin's *Lehrbuch*, is the conventional method of defining the lambda for the purposes of measurement. Sergi states that there are at least four other possible lambdas placed more posteriorly in, or near, the middle line between the ossicles, L_2, L_3, L_4 and L_5. The length of the arc between L_1 and L_5 is stated to be 35 mm.; Sergi has discussed the subject in his paper entitled: "Ossicini fontanellari della regione del lambda nel cranio di Saccopastore e nei crani neandertaliani."* He also gives in this paper a natural-size drawing of the occipital view of the skull, showing the accessory bones. The measurement given for the occipital arc in the Saccopastore skull is 142 mm. and that for its supra-inial segment 90 mm. That these may be considered exceptionally large measurements is clearly illustrated by comparing them with the measurements of the corresponding characters in the La Chapelle-aux-Saints skull—the largest male skull of the Neanderthal type yet described. In this specimen the occipital arc is about 117 mm. and the supra-inial arc 74 mm. In an exceptionally large modern skull with a capacity of 2450 c.c. and a pre-interparietal bone present the length of the occipital arc was found to be 153 mm., merely 11 mm. more, and the length of the supra-inial arc 86 mm., i.e. 4 mm. less than the value for the specimen from Rome.

Though the parietal arc and chord in the Saccopastore specimen are unusually short, the index of curvature of the parietal arc is much the same as that estimated for the London skull and as that recorded for the La Quina skull.

The curvature of the occipital bone as a whole in the Saccopastore skull, as shown by the values of Pearson's occipital index and the chord-arc index, $100 \times S_3'/S_3$, is much greater than in the London skull and also rather greater than in the Gibraltar skull. The curvature of the supra-inial segment of the occipital arc in the Saccopastore, as in the La Quina specimen, is greater than in the London, though the Gibraltar agrees closely with the last-named specimen in this feature. The curvature of the infra-inial arc in the Saccopastore does not differ greatly from that in the London skull which also agrees closely with that in the Gibraltar skull.

As already mentioned, the biasterionic breadth in the Saccopastore specimen

* *Rivista di Antropologia*, 1934, Vol. 30.

¶ Estimated to be 75 mm. by Sir Arthur Keith.

is almost identical with the corresponding measurement in the London skull. In two of the three indices tabulated of which the biasterionic breadth is a component the Saccopastore skull does not differ greatly from the London skull. Thus 100 B'''/S_3' in the Saccopastore is 112, in the London skull 114; 100 B'''/B in the Saccopastore is 81, in the London skull 79. The third index 100 B'''/S_2' in the Saccopastore, on the other hand, is 139, whereas the corresponding index in the London skull is 113. This divergence is wholly due to the very short parietal chord (S_2') in the Saccopastore specimen.

The comparison of the London skull with the Gibraltar and La Quina specimens will be dealt with more briefly. The Gibraltar skull is much longer and rather broader, the La Quina skull much longer and narrower than the London. In biasterionic breadth, as in maximum parietal breadth, the London skull lies intermediately to these two Neanderthal skulls.

The lengths of the parietal arc and chord are not available for the Gibraltar skull, but in the La Quina specimen they are almost the same as the corresponding characters in the London skull, and the indices of curvature of the parietal bone (100 S_2'/S_2) in the two are practically identical. In the pronounced flatness of the parietal segment of its sagittal arc the London skull undoubtedly presents a definite Neanderthaloid feature.

In the Gibraltar skull the length of the occipital arc (S_3) is much less than the corresponding arc in the London skull, and still less than that in the Saccopastore. The curvature of the occipital arc in the Gibraltar is rather greater than in the London. The lambda-inion arc and chord in the Gibraltar specimen are also much less than those shown in the London skull, but they are reduced in proportion as the curvature of the arc agrees closely with that in the London skull. In the La Quina specimen the lengths of the lambda-inion arc and chord do not differ notably from the corresponding measurements in the Gibraltar skull but the curvature of the arc is rather greater. The inion-opisthion arc cannot be measured in the La Quina specimen, but in the Gibraltar skull the arc and chord are about 5 mm. greater than the corresponding characters in the London skull; the ratio of chord to arc, i.e. the index of curvature, in the two skulls is thus almost identical.

Turning to the three indices of which the biasterionic breadth is a component, two only are calculable for each of the skulls from Gibraltar and La Quina. The biasterionic breadth as a percentage of the maximum parietal breadth in the London skull is identical with the corresponding ratio in the Gibraltar skull, and is but two units less than that in the La Quina specimen. The biasterionic breadth-parietal chord index (100 B'''/S_2') is not available for the Gibraltar specimen but is measurable in the La Quina skull. In this skull it is rather less than in the London specimen, but the difference is less than four units. The biasterionic breadth-occipital chord index (100 B'''/S_3') is not available for the La Quina specimen. In the Gibraltar skull it is much greater than in the London skull.

The difference is mainly due to the great difference in the length of the occipital chord (S'_3); this is 15 mm. shorter in the Gibraltar than in the London skull.

The greater curvature of the occipital bone in the Gibraltar skull than in the London is also brought out by the size of the occipital (lambda-inion-opisthion) angle. In the London skull the angle is 117° but in the Gibraltar skull it is 10° less.

(*e*) *Comparison with the Piltdown Skull.* It must be emphasized that no claim is made that any measurements made on the reconstructed Piltdown skull are to be considered more than rough approximations to the true values of the characters. If resemblances between its general form and that of the London skull can be recognized by superimposition of their outline tracings in different planes, it might reasonably be expected that some indication of these resemblances will be indicated by a comparison of the measurable characters and their relative proportions. As will be seen by reference to Table III, both the extent and curvature of the occipital segment of the sagittal arc are in close agreement with the corresponding characters in the London skull, but not closer than those for the mean skull in the Scottish anomalous group (Table II). The supra-inial segment of the occipital arc in the Piltdown skull, though it is definitely less extensive than that of the London skull, shows a similar degree of curvature. The infra-inial arc, on the other hand, appears to be definitely longer than, though also little different in curvature from, that in the London skull. The ratio of the supra-inial segment of the occipital arc to the arc as a whole (56 per cent) in the Piltdown thus differs considerably from that in the London skull; it is in fair agreement, however, with the average proportion found in the normal modern Scottish series, practically coinciding with that in the anomalous Scottish group and differing by not more than three units from the corresponding character in the Gibraltar skull.

The angle between the inion-opisthion chord and the line of the subcerebral plane, the size of which has been taken as a crude index of the fullness of curvature in the lower occipital region, is about 42°, i.e. the same as in the London skull. It must be recollected, however, that in the latter specimen the inion-opisthion chord is relatively short. In the Scottish skull, for which the mean value of the inion-opisthion chord agrees closely with that in the Piltdown skull, the corresponding angle is also practically the same as in the London.

The parietal segment of the sagittal arc in the Piltdown skull in extent and curvature seems to correspond closely with the corresponding mean values in the modern Scottish series; it is not relatively short and flat as in the London skull. The biasterionic breadth in the Piltdown skull appears to be definitely in excess of the corresponding measurement in the London skull, and the proportions borne by this diameter to the maximum parietal breadth and to the occipital chord, respectively, in it are also appreciably greater than the corresponding ratios in the London. The ratio of biasterionic breadth to parietal chord is, however, almost the same in the two specimens.

So far as available measurements of characters and their proportions give any material indication of relationship, the London skull does not appear to show any closer affinity with the Piltdown type than with the modern Scottish type.

(*f*) *Comparison with the Swanscombe Skull.* Some of the characters in the London skull fragment can also be compared with those of the prehistoric skull recently discovered at Swanscombe in Kent by Mr A. T. Marston. The Swanscombe skull is now admitted by all the leading authorities to have been found in the middle gravels of the 100 ft. terrace of the Thames and to be of Acheulean date. It consists of the complete occipital and left parietal bones, which articulate with one another very accurately. The parts preserved are much the same as in the London skull, except that in the latter the occipital and left parietal are not quite complete and a small portion of the right parietal is present. The bones in the Swanscombe skull, like those of the Piltdown skull, are much thicker than the cranial wall in the London skull, in which, as already mentioned, the thickness is much the same as in the average modern female skull. But both parietal and occipital bones as thick as those of the Swanscombe specimen are occasionally found in modern skulls with no evidence of disease.

The measurements of the Swanscombe specimen that can be compared with those of the London fragment are shown in Table III. In Figs. 5, 6 and 7 are also shown superimpositions of the dioptographic contour tracings of the London and Swanscombe skulls (the latter from a cast) in three planes, the sagittal, the horizontal and the transverse. In the profile contours the asterions and the lines indicating an approximation to the subcerebral plane, as determined by the direction of the left parieto-mastoid suture, have been made coincident. In the horizontal tracing the skulls are oriented in the subcerebral plane and the bregmatic points have been made to coincide. In the transverse maximum contour tracing the skulls are oriented at right angles to the subcerebral plane and the midpoints of the biasterionic diameters are coincident.

The maximum parietal breadth (*B*) in the Swanscombe skull (obtained by assuming the transverse contour of the right side to be the mirror image of that of the left at its widest part) is much the same as in the London skull, the respective measurements being 142 and 144 mm. In biasterionic breadth (*B'''*) there is, however, an appreciable difference, the Swanscombe specimen being, like the Piltdown, wider in this region by about 7–9 mm. In this feature the Swanscombe skull, like the London (p. 304), probably exceeds the range of values found in skulls of modern type and of much the same size in their other principal dimensions. These differences are well illustrated in Fig. 7. The basio-bregmatic height (*H'*) is not determinable accurately in the London skull, but in so far as it can be estimated from the other characters as approximately 120 mm. it seems to be definitely less than the corresponding height of the Swanscombe, which is 126 mm. (see Fig. 5). The latter is rather in excess of the average basio-

London skull
Swanscombe skull

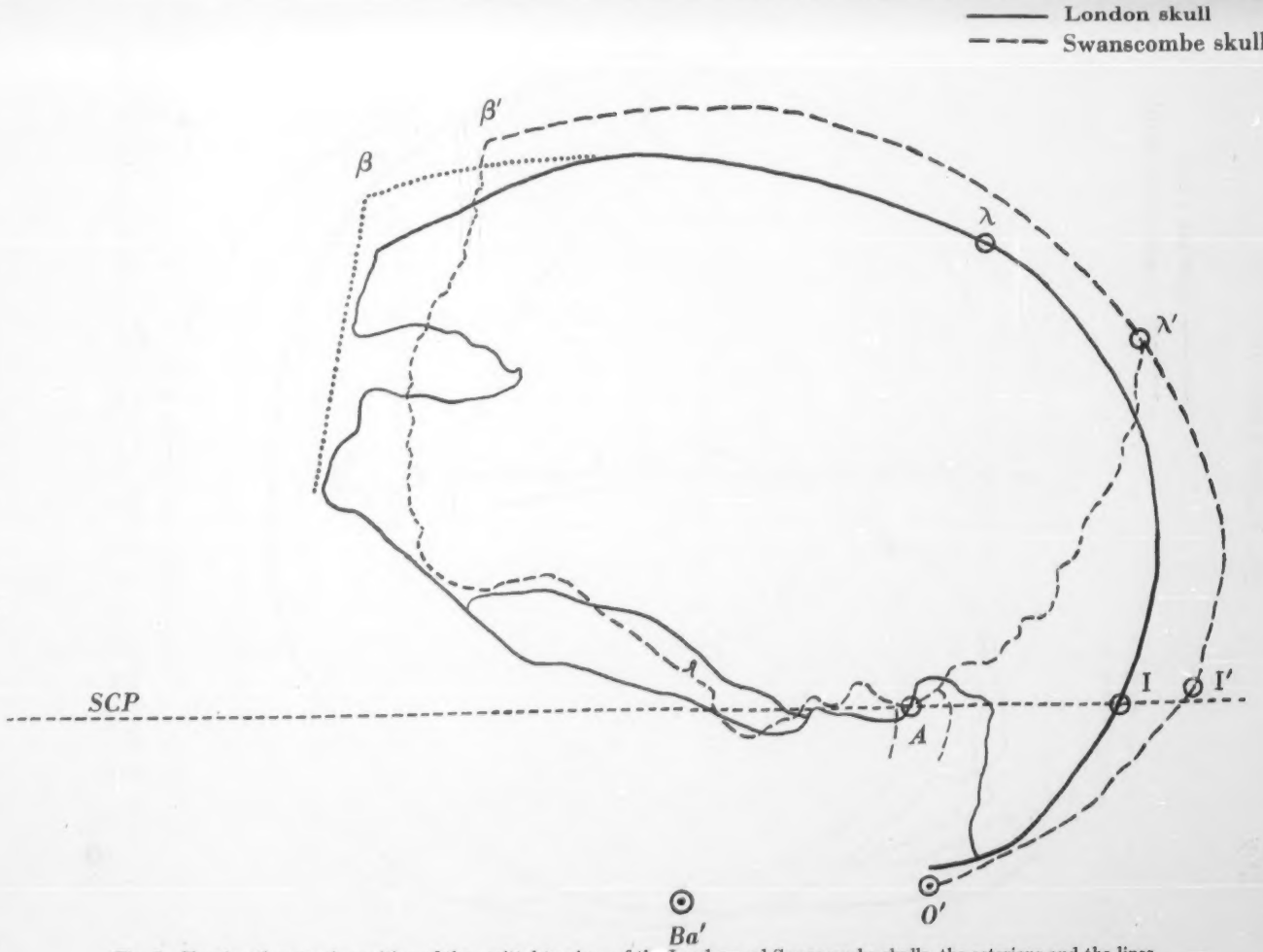

Fig. 5. Showing the superimposition of the sagittal tracings of the London and Swanscombe skulls, the asterions and the lines indicating the subcerebral plane being made coincident.

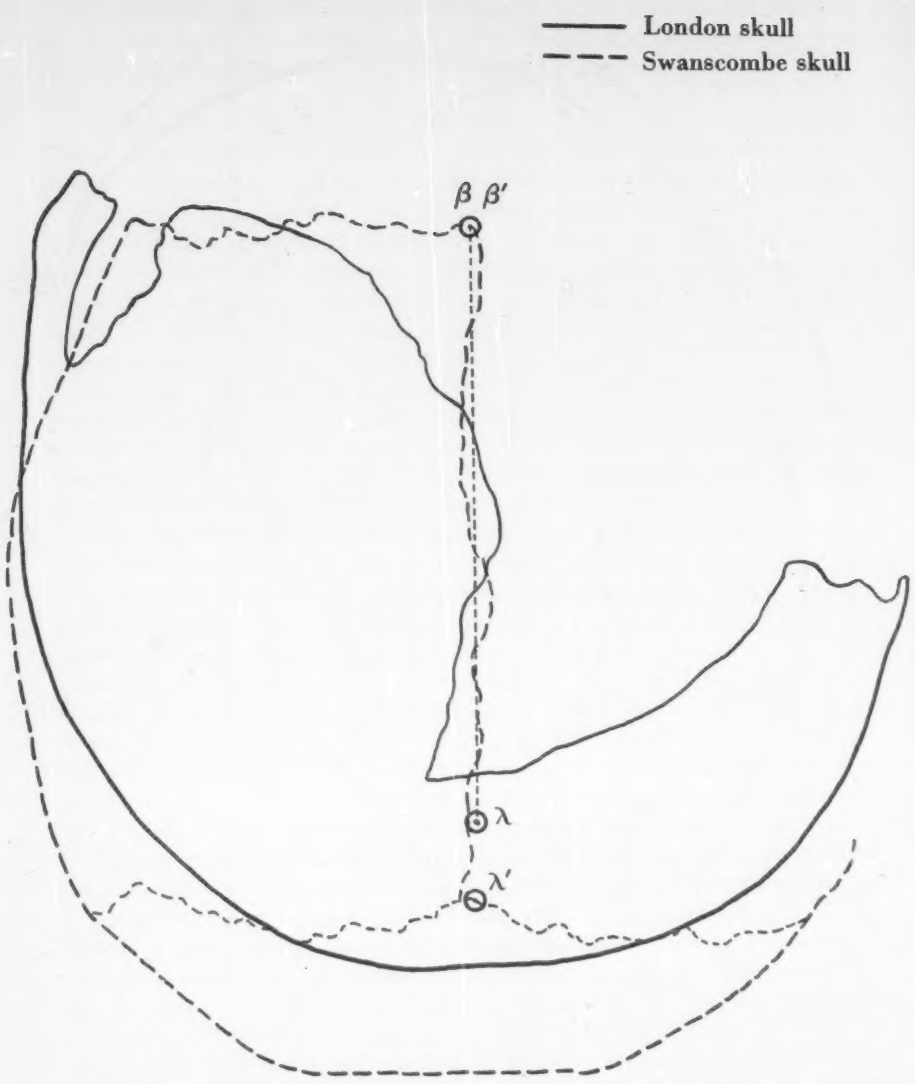

Fig. 6. Showing the superimposition of the maximum horizontal tracings of the London and Swanscombe skulls when the skulls are oriented in the subcerebral plane, and the bregmatic points are made coincident.

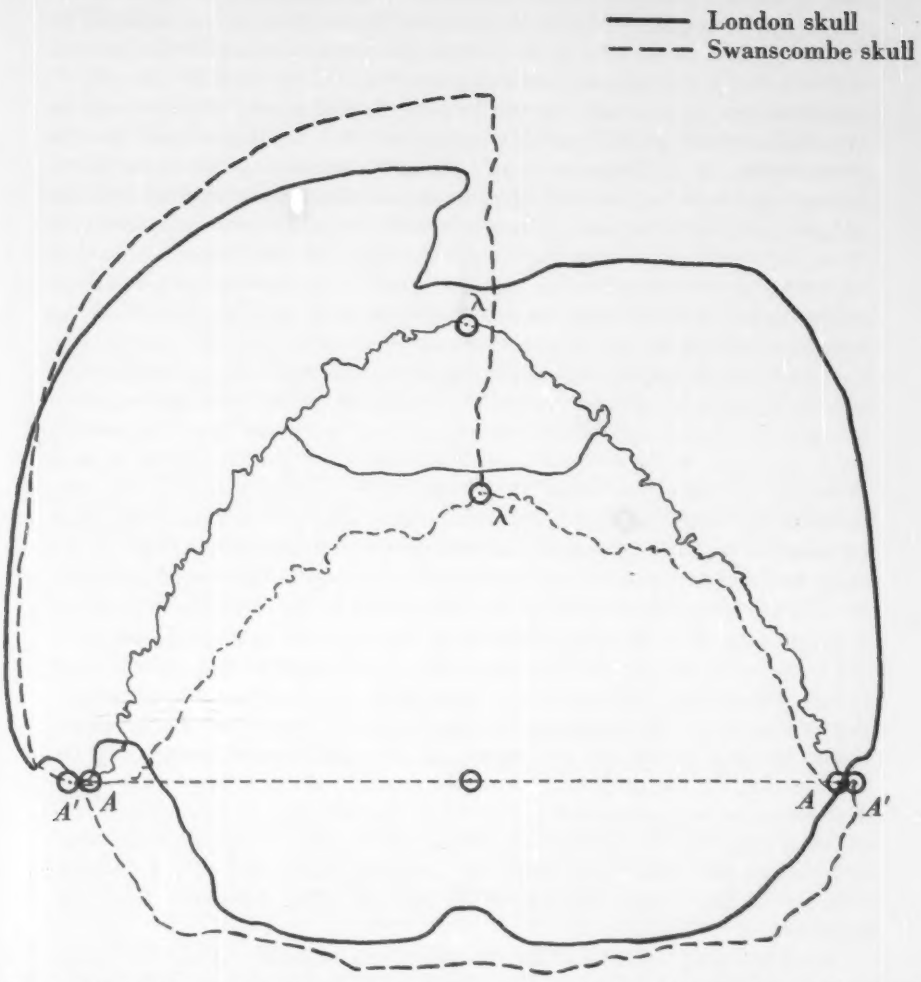

Fig. 7. Showing the superimposition of the maximum transverse tracings of the London and Swanscombe skulls when the skulls are oriented at right angles to the subcerebral plane and the mid-points of the biasterionic diameters are made coincident.

bregmatic height in the Farringdon Street female seventeenth-century Londoners (Table II) and coincides with the average for the modern West Scottish female. The maximum length (L) is not determinable accurately in the Swanscombe skull, but the length that results from the completion of the frontal region of the sagittal contour of the skull in the manner that seems not improbable from the segment that is extant is approximately 180 mm. This is much the same as the estimated maximum length for the London skull. The cephalic indices in the two skulls are thus probably in close agreement, 80 in the London and 79 in the Swanscombe. It is of interest to note that the maximum width of the Saccopastore skull is the same as in the Swanscombe skull and the maximum length is 181 mm., i.e. nearly the same, giving a cephalic index of 78·5 as compared with 79·0. The incomplete horizontal contour tracing of the Swanscombe skull suggests that when complete the specimen would have shown some post-orbital constriction, a feature that does not appear to be so clearly indicated in the London skull (Fig. 6).

Turning to the sagittal contours, the parietal arc in the Swanscombe is longer, and the occipital arc shorter, than in the London skull, but the lengths of these two arcs combined do not differ in the two skulls by more than 1 mm. The parietal arc is not quite so flat in the Swanscombe skull as in the London skull, as is shown by the respective values of the index ($100\ S_2'/S_2$) of 96·2 and 93·0. The occipital arc, though shorter in the Swanscombe specimen, has much the same curvature as in the London skull, the indices of occipital curvature ($100 \times S_3'/S_3$) being 81·7 and 81·3 and the values of Pearson's occipital index being 58·4 and 58·1. This similarity in curvature is also brought out by the fairly close agreement of the occipital (lambda-inion-opisthion) angles in the two skulls, 114° and 117°, and is shown in the contour tracings in Fig. 5. It is interesting to note that though both the occipital arc and the parietal arc of the Swanscombe skull are shorter than in the Piltdown, yet the curvatures of the corresponding bones are almost identical in the two specimens. In the superimposed sagittal contour tracings (Fig. 5), the projected line of the coronal suture in the Swanscombe skull seems to be approximately parallel to the direction in which the corresponding suture in the London skull is assumed to run, though when oriented as shown in the subcerebral plane, the antero-posterior axis of the foramen magnum in the former appears to be slightly tilted backward from the horizontal.

The supra-inial segment of the occipital arc is shorter and the infra-inial segment relatively longer in the Swanscombe than in the London skull. These differences may be related to the presence of the pre-interparietal bone in the London specimen, but the indices of curvature in the corresponding segments in the two skulls are of much the same order, as shown by the values 92·1 and 92·7, and 98·1 and 97·6. The proportion which the lambda-inion arc forms of the total occipital arc is less in the Swanscombe than in the London skull, 55 per cent. as

compared with 67 per cent. This ratio in the Swanscombe skull is in close agreement with that in the Piltdown.

As might be expected from the difference in biasterionic breadth, the ratio of the biasterionic breadth to the maximum parietal breadth is appreciably greater in the Swanscombe skull than in the London, the values being 85 and 79. With a greater biasterionic breadth and a shorter occipital chord in the Swanscombe the ratio 100 × biasterionic breadth/occipital arc in this specimen is also greater than in the London. On the other hand, the biasterionic breadth and the parietal chord in the Swanscombe exceed the corresponding measurements in the London specimen in much the same proportion, so that the ratio 100 × biasterionic breadth/parietal chord almost coincides in the two skulls, the indices being 114 and 113. This ratio in the Piltdown skull is also in close agreement with that in the Swanscombe.

So far as a study of the comparable measurements and their proportions in the Swanscombe and London skull fragments permits any inferences to be drawn, there would appear to be no unequivocal evidence that the Swanscombe skull shows any greater divergence from the modern type than does the London specimen.

6. *Summary and Conclusions.* The main inferences that can be drawn from the detailed comparisons that have been made may be summarized briefly:

1. From an anatomical point of view the London skull fragment apparently possesses no features other than the presence of a pre-interparietal bone which would be at all exceptional if found in a specimen of modern type.

2. Comparison of the fragment with the female skulls of well-authenticated Aurignacian and Solutrean date from Europe—considered as a group and individually—reveal the close resemblance between it and the skulls from Solutré. So far as can be judged from the available measurements, the London skull would appear to be indistinguishable in type from the skull usually designated Solutré V (1924).

3. Amongst the British skulls or fragments of skulls of reputed late palaeolithic or earlier date, excluding for the moment the Piltdown and Swanscombe skulls, the specimen that seems to suggest most strikingly a similarity of type with the London skull is the fragment of reputed late Acheulean date discovered in the vicinity of Bury St Edmunds. The most reasonable prediction of the missing parts of the vault in one fragment is in such close agreement with what is extant in the other that, while it is possible that the similarity may merely be a coincidence, it seems rather to support the view that the two fragments represent calvariae that were essentially similar in their proportions and general form.

4. From the differences that are observed in the mean measurements of two modern Scottish female groups—one (the normal) without and the other (the

anomalous) with interparietals—there seems to be a reasonable basis for the inference that the presence of the pre-interparietal bone in the London skull is probably largely responsible for the rather unusual formation of its occipital arc, in respect of extent and curvature, as well as for the relative shortness of its parietal arc.

5. Of the differences that are observed in the measurements of the characters in the London skull and the corresponding mean values in the Scottish series and in the closely related series of recent Londoners, only a few are of such a magnitude as to indicate that the measurements in the palaeolithic specimen may reasonably be considered exceptional for these series. Amongst these, the principal are the relatively long supra-inial and short infra-inial segments of the occipital arc and the relatively short and flat parietal arc. A disproportion in the segmental lengths of the occipital arc is not a primitive feature, as their relative proportions in the Gibraltar and the Piltdown skulls are identical with that found in the Scottish group with interparietals. It is possible that the relatively short infra-inial arc in the London skull may be to some extent compensatory to the long supra-inial arc.

6. Comparison of the available measurements and their proportions in the London fragment with those of the corresponding characters in the Neanderthal female skulls from Gibraltar, La Quina and Saccopastore seems to indicate that, in its general form and dimensions, it resembles the last-named specimen most closely. In the unusual flatness of the parietal segment of the sagittal arc, it presents a definite Neanderthaloid feature, and possibly also in its relatively great biasterionic breadth, though the latter measurement is of the order occasionally found in modern types of skull.

7. In its measurements and proportions there is no unequivocal evidence that the London skull has a closer kinship with the Piltdown skull than with the modern Scottish female type. In the extent and curvature of its parietal arc, as well as in the proportions of biasterionic breadth to maximum parietal breadth and of biasterionic breadth to length of occipital chord, it is nearer to the Scottish than the Piltdown type, while in fullness of curvature of the lower occipital region it is in quite as close agreement with the one type as the other.

8. So far as can be ascertained from the comparison of measurements of the available characters and their proportions, there is no evidence that the Swanscombe skull shows a greater divergence from the modern type than does the London specimen.

It may be stated, finally, that the present study seems to indicate conclusively that the London skull is of the modern type and resembles closely in its general form that of the upper palaeolithic period found at Solutré, and especially the specimen designated Solutré V (1924). So far as can be judged from the fragments

A. The London skull from the lateral aspect.

B. The London skull from the vertical aspect.

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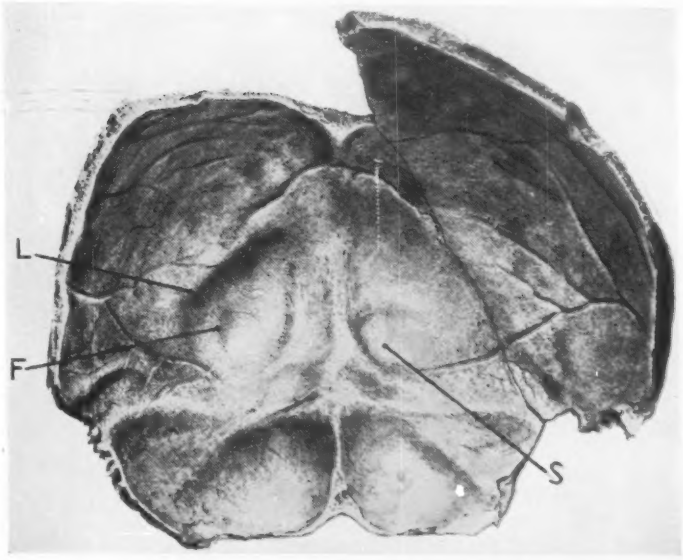
A. The London skull from the occipital aspect.



B. The London skull from the internal aspect.

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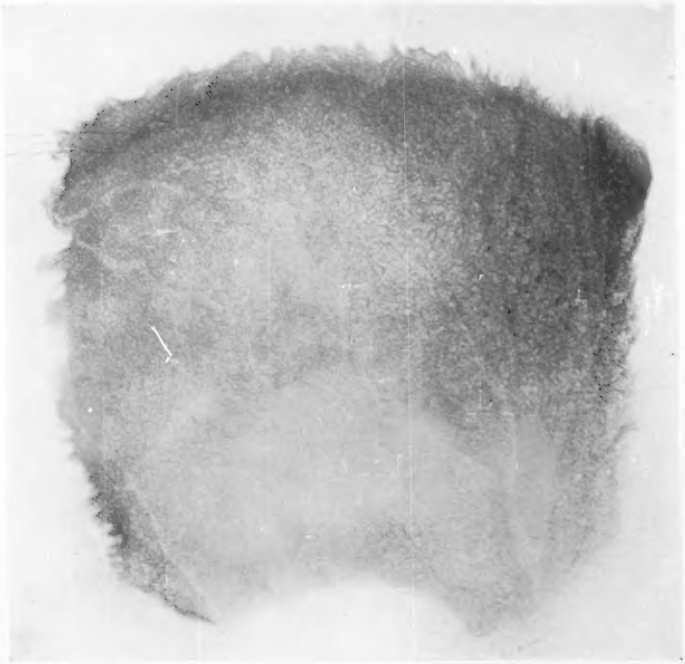
A Radiograph of parietal bone of the London skull.



B. Radiograph of parietal bone of seventeenth-century Londoner.

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that are preserved, it is also remarkably like the skull of the same presumptive age—i.e. late Acheulean or pre-Mousterian—found at Bury St Edmunds, Suffolk, and sometimes referred to as the Westley skull.

DESCRIPTION OF PLATES

Plate I. A. Showing the London skull from the lateral aspect when oriented in the subcerebral plane.

B. Showing the London skull from the vertical aspect when oriented in the subcerebral plane.

Plate II. A. Showing the London skull from the occipital aspect when oriented in the subcerebral plane. The line of fusion of the pre-interparietal bone is seen.

B. Showing the interior of the London skull viewed from the front (from a drawing by Mr A. K. Maxwell). *L*, crista lunata; *F*, deep fossa corticis striatae of the right side; *S*, small fossa corticis striatae of the left side.

Plate III. Showing a comparison of the radiograph of the parietal bone of the London skull (A) with that of a seventeenth-century Londoner (B) of much the same thickness. The high degree of mineralization of the former is clearly indicated. The densities of the two bones should be truly comparable, as the pictures were taken on the same film and print.

SIGNIFICANCE TESTS WHICH MAY BE APPLIED TO SAMPLES FROM ANY POPULATIONS

III.* THE ANALYSIS OF VARIANCE TEST

By E. J. G. PITMAN
University of Tasmania

1. *The two forms of the analysis of variance test.* The main title of this paper is, perhaps, not strictly accurate, for, in that form of the analysis of variance test discussed here, the observed numbers are not really regarded as a sample from a larger population, though, in an actual application of the test, the classification determined by "treatment" is only one of many possible. The essential point is that no assumptions are made about forms of populations. This, and the methods employed, link the paper naturally with the preceding papers (1,2) of this series.

As one of a series, this paper was planned many months ago; but it was not written until June of this year, 1937. It arrived in England just about the time when B. L. Welch's paper (5) on the analysis of variance appeared. While some of its results are anticipated by Welch, the present paper goes deeper into the randomization theory of the simplest type of analysis of variance test.

The principles of this test may be briefly summarized as follows. Several batches, each consisting of n individuals, are taken and the individuals of a batch subjected to n different treatments, the allocation of the treatments to the individuals of a batch being determined by chance. Each individual is then measured, and we wish to determine whether the differences in treatment have produced any real differences in the character measured. The batches might, for example, be the blocks in an agricultural experiment, and the individuals the plots into which each block is subdivided.

If there are m batches, our observations consist of m sets of numbers,

$$a_1, a_2, \ldots, a_n,$$
$$b_1, b_2, \ldots, b_n,$$
$$\ldots\ldots\ldots\ldots,$$

where $a_r, b_r, \ldots$ are the results for the m individuals subjected to treatment r. We assume that

$$a_r = A + T_r + x_{ar}, \quad b_r = B + T_r + x_{br}, \quad \text{etc.},$$

where A denotes the result of some cause which affects equally all the individuals of the first batch, T_r denotes the effect of treatment r, and the third term x_{ar} arises from the variability among the individuals of a batch, errors in measure-

* For the previous papers of this series see (1) and (2) in list of references.

ment, and other accidents affecting particular individuals. The analysis of variance is

$$S = S_B + S_T + S_E,$$

where S denotes the total sum of squares,

S_B is the sum of squares due to batches, and is independent of $T_1, T_2, \ldots$,

S_T is the sum of squares due to treatments, and is independent of $A, B, \ldots$,

S_E is the residual sum of squares, and is independent of both $A, B, \ldots$, and $T_1, T_2, \ldots$.

Differences in the values of the T tend to increase the value of S_T while not affecting the value of S_E; hence large values of S_T/S_E are regarded as significant. The question then is, does the observed value of S_T/S_E indicate that the T are not all equal, or is it such as might easily arise by chance when the T are all equal?

It can be shown that if the x are independent chance variables each with the same normal distribution of standard deviation σ, and if $T_1 = T_2 = \ldots$, then S_T/σ^2 and S_E/σ^2 are independent chance variables distributed like χ^2 with degrees of freedom $n-1$ and $(m-1)(n-1)$ respectively. Hence

$$W = \frac{S_T}{S_T + S_E} = \frac{S_T}{S - S_B},$$

which is a monotonic increasing function of S_T/S_E, has a

$$B\{\tfrac{1}{2}(n-1), \tfrac{1}{2}(m-1)(n-1)\}$$

distribution. This gives the usual test based on the above assumptions, though, in practice, some monotonic function of W such as Fisher's z, which is

$$\tfrac{1}{2}\log_e\{(m-1)W/(1-W)\},$$

is often employed. It should be noted that the theoretical repetitions which determine this distribution of W are repetitions of the whole experiment, and that the x values will be different samples from the same normal population.

The problem of testing the null hypothesis, that the T are all equal, has been tackled without making any assumptions about the x. If the null hypothesis is true, the observed value of W is the result of the chance allocation of the different treatments to the different individuals in the batches. We may imagine repetitions of the same experiment with the same batches and the same individuals, each with its corresponding x unaltered, but with different allocations of the treatments to the individuals in the various batches. If the T are all equal, the observed numbers $a_1, a_2, \ldots, b_1, b_2, \ldots$ will remain the same but will be arranged in different orders. There are $(n!)^{m-1}$ ways in which the numbers may be grouped into n groups each containing one and only one number from each batch, so that W may take $(n!)^{m-1}$ values, some of which may happen to coincide with one another. As the allocation of treatments to individuals is determined by chance,

all such groupings are equally likely, and so, therefore, are the corresponding values of W. To test the null hypothesis in this way, we must know this distribution of W, which is entirely determined by the observed numbers a_1, a_2, etc. We can then determine the probability that, if the null hypothesis were true, a value of W as great as, or greater than, that observed would be obtained. If this probability is small, say less than 0·05, we consider that the observed value of W is significant and that the differences in treatment have produced real differences in effect.

It is frequently stated that, for cases which commonly occur in certain fields of work, this distribution of W is approximately the same as the other distribution of W, that is, that this distribution of W is approximately a

$$B\{\tfrac{1}{2}(n-1), \tfrac{1}{2}(m-1)(n-1)\}$$

distribution. Eden and Yates(3), by a sampling process, showed that there was very good agreement between the B-distribution and the actual distribution of W in a case which they investigated. In order to discuss the question we shall obtain the first four moments of the exact distribution of W. Welch, in the paper(5) referred to above, obtained expressions for the first two moments.

2. *The moments of* W. Since

$$W=\frac{S_T}{S_T+S_E}=\frac{S_T}{S-S_B}$$

is independent of the quantities A, B, etc., we may assume these to have such values that the mean of each batch is zero. The mean of the whole set of mn numbers is then zero, and also $S_B=0$. We then have

$$W=\frac{S_T}{S}=\frac{\frac{1}{m}\sum_{r=1}^{n}(a_r+b_r+\ldots)^2}{\sum_{r=1}^{n}a_r^2+\sum_{r=1}^{n}b_r^2+\ldots}.$$

The numbers

$$a_1, a_2, \ldots, a_n$$

are always a permutation of the same set of n numbers; the different values of W are obtained from the different permutations of the numbers in each batch. We shall denote the second, third, and fourth moments of the a by α_2, α_3, α_4, and their second, third, and fourth k-statistics (see (4), p. 75) by α_2', α_3', α_4'. We shall write

$$R_{ab}=a_1b_1+a_2b_2+\ldots+a_nb_n.$$

There are ${}_mC_2=M$, say, of these expressions, which it will sometimes be convenient to denote by

$$R_1, R_2, \ldots, R_M.$$

Putting

$$U=R_1+R_2+\ldots+R_M,$$

we have

$$\Sigma(a_r+b_r+\ldots)^2=\Sigma a_r^2+\Sigma b_r^2+\ldots+2U,$$

and therefore $$W=\frac{1}{m}+\frac{2U}{mn\Sigma\alpha_2}.$$

The moments of R_{ab} are as follows:

$$E(R_{ab})=0,$$

$$E(R_{ab}^2)=\frac{n^2\alpha_2\beta_2}{n-1},$$

$$E(R_{ab}^3)=\frac{n^3\alpha_3\beta_3}{(n-1)(n-2)}=\frac{(n-1)(n-2)\alpha_3'\beta_3'}{n},$$

$$E(R_{ab}^4)=\frac{3n^4\alpha_2^2\beta_2^2}{(n-1)(n+1)}+\frac{(n-1)(n-2)(n-3)\alpha_4'\beta_4'}{n(n+1)}.$$

These may be obtained directly without much labour; thus, for example,

$$\begin{aligned}E(R_{ab}^4)&=E\{(\Sigma a_p b_p)^4\}\\&=E\{\Sigma a_p^4 b_p^4+4\Sigma a_p^3 a_q b_p^3 b_q+6\Sigma a_p^2 a_q^2 b_p^2 b_q^2\\&\qquad+12\Sigma a_p^2 a_q a_r b_p^2 b_q b_r+24\Sigma a_p a_q a_r a_s b_p b_q b_r b_s\}\\&=nE(a_1^4)E(b_1^4)+4n(n-1)E(a_1^3a_2)E(b_1^3b_2)+\text{etc.}\end{aligned}$$

The mean value of such an expression as $a_1^3a_2$ is easily obtained, for we have

$$0=\Sigma a_p\Sigma a_p^3=\Sigma a_p^3a_q+\Sigma a_p^4=n(n-1)E(a_1^3a_2)+n\alpha_4,$$

and therefore $$E(a_1^3a_2)=-\alpha_4/(n-1).$$

Proceeding in this way and then collecting terms, we obtain the result given above.

Before attempting to find the moments of

$$U=R_1+R_2+\ldots+R_M,$$

we must note that any two of the R are independent, and therefore

$$E(R_pR_q)=0.$$

Also, any three of the R are independent unless the three form a set like

$$R_{ab},\ R_{bc},\ R_{ca},$$

which (in the double-suffix notation) involves only three suffixes. Hence, in particular, for three R not related in this way,

$$E(R_pR_qR_r)=0.$$

In general, if in any product of any number of the R one suffix (in the double suffix notation) is not repeated, the corresponding R will be independent of the other R in the product, and therefore the mean value of the product will be zero.

This implies that the mean value of the product of four R will be zero unless the suffixes form a closed chain like

$$R_{ab},\ R_{bc},\ R_{cd},\ R_{da}.$$

$$E\,(U)=E\,(\Sigma R_p)=\Sigma E\,(R_p)=0.$$

$$E\,(U^2)=E\,(\Sigma R_p^2+2\Sigma R_p R_q)=\Sigma E\,(R_p^2)$$

$$=\frac{n^2}{n-1}\,\Sigma a_2\beta_2.$$

$$E\,(U^3)=E\,(\Sigma R_p^3+3\Sigma R_p^2 R_q+6\Sigma R_p R_q R_r)$$

$$=\Sigma E\,(R_p^3)+6\Sigma E\,(R_{ab}R_{bc}R_{ca}).$$

Now $$E\,(R_{ab}R_{bc}R_{ca})=E\,(\Sigma a_p b_p.\Sigma b_p c_p.\Sigma c_p a_p)$$

$$=E\,\{[\Sigma b_p^2 a_p c_p+\Sigma b_p b_q\,(a_p c_q+a_q c_p)]\,\Sigma c_p a_p\}$$

$$=E\,\{[E\,(b_1^2)\,\Sigma a_p c_p+E\,(b_1 b_2)\,\Sigma\,(a_p c_q+a_q c_p)]\,\Sigma c_p a_p\}.$$

But $$\Sigma a_p c_p+\Sigma\,(a_p c_q+a_q c_p)=\Sigma a_p\Sigma c_p=0,$$

therefore $$\Sigma\,(a_p c_q+a_q c_p)=-\Sigma a_p c_p.$$

Thus $$E\,(R_{ab}R_{bc}R_{ca})=\{E\,(b_1^2)-E\,(b_1 b_2)\}\,E\,\{(\Sigma a_p c_p)^2\}$$

$$=\left(\beta_2+\frac{\beta_2}{n-1}\right)\frac{n^2\alpha_2\gamma_2}{n-1}=\frac{n^3\alpha_2\beta_2\gamma_2}{(n-1)^2}.$$

Since $$\Sigma E\,(R_p^3)=\frac{(n-1)\,(n-2)}{n}\,\Sigma\alpha_3'\beta_3',$$

this gives $$E\,(U^3)=\frac{6n^3}{(n-1)^2}\,\Sigma\alpha_2\beta_2\gamma_2+\frac{(n-1)\,(n-2)}{n}\,\Sigma\alpha_3'\beta_3'.$$

$$E\,(U^4)=E\,(\Sigma R_p^4+4\Sigma R_p^3 R_q+6\Sigma R_p^2 R_q^2+12\Sigma R_p^2 R_q R_r+24\Sigma R_p R_q R_r R_s)$$

$$=\Sigma E\,(R_p^4)+6\Sigma E\,(R_p^2)\,E\,(R_q^2)+12\Sigma E\,(R_{ib}^2 R_{bc}R_{ca})$$

$$+24\Sigma E\,(R_{ab}R_{bc}R_{cd}R_{da}).$$

$$\Sigma E\,(R_p^2)\,E\,(R_q^2)=\tfrac{1}{2}\,\{[\Sigma E\,(R_p^2)]^2-\Sigma\,[E\,(R_p^2)]^2\}$$

$$=\frac{n^4}{2\,(n-1)^2}\,\{[\Sigma\alpha_2\beta_2]^2-\Sigma\alpha_2^2\beta_2^2\}.$$

$$E\,(R_{ab}^2 R_{bc}R_{ca})=E\,\{(\Sigma a_p b_p)^2.\Sigma b_p c_p.\Sigma c_p a_p\}$$

$$=E\,\{(\Sigma a_p b_p)^2\,(\Sigma c_p^2 a_p b_p+\Sigma c_p c_q\,(a_p b_q+a_q b_p))\}$$

$$=E\,\{(\Sigma a_p b_p)^2\,(E\,(c_1^2)-E\,(c_1 c_2))\,\Sigma a_p b_p\}$$

$$=\{E\,(c_1^2)-E\,(c_1 c_2)\}\,E\,\{(\Sigma a_p b_p)^3\}$$

$$=\frac{n\gamma_2}{n-1}\,\frac{(n-1)\,(n-2)\,\alpha_3'\beta_3'}{n}$$

$$=(n-2)\,\alpha_3'\beta_3'\gamma_2.$$

$$E(R_{ab}R_{bc}R_{cd}R_{da}) = E(\Sigma a_p b_p . \Sigma b_p c_p . \Sigma c_p d_p . \Sigma d_p a_p)$$
$$= E\{(\Sigma b_p^2 a_p c_p + \Sigma b_p b_q (a_p c_q + a_q c_p))(\Sigma d_p^2 a_p c_p + \Sigma d_p d_q (a_p c_q + a_q c_p))\}$$
$$= E\{(E(b_1^2) - E(b_1 b_2))\Sigma a_p c_p . (E(d_1^2) - E(d_1 d_2))\Sigma a_p c_p\}$$
$$= \{E(b_1^2) - E(b_1 b_2)\}\{E(d_1^2) - E(d_1 d_2)\} E\{(\Sigma a_p c_p)^2\}$$
$$= \frac{n^4}{(n-1)^3}\alpha_2\beta_2\gamma_2\delta_2 .$$

Since four letters can be arranged in a closed chain in three ways, this gives

$$E\{\Sigma R_{ab}R_{bc}R_{cd}R_{da}\} = \frac{3n^4}{(n-1)^3}\Sigma\alpha_2\beta_2\gamma_2\delta_2 .$$

Thus
$$E(U^4) = \frac{3n^4}{(n-1)(n+1)}\Sigma\alpha_2^2\beta_2^2 + \frac{(n-1)(n-2)(n-3)}{n(n+1)}\Sigma\alpha_4'\beta_4'$$
$$+ \frac{3n^4}{(n-1)^2}\{(\Sigma\alpha_2\beta_2)^2 - \Sigma\alpha_2^2\beta_2^2\}$$
$$+ 12(n-2)\Sigma\alpha_3'\beta_3'\gamma_2 + \frac{3.24n^4}{(n-1)^3}\Sigma\alpha_2\beta_2\gamma_2\delta_2 .$$

Finally, for the moments of

$$W = \frac{1}{m} + \frac{2U}{mn\Sigma\alpha_2},$$

we have* $E(W) = \frac{1}{m}$

$$E\{(W-\overline{W})^2\} = \frac{4}{m^2(n-1)}\frac{\Sigma\alpha_2\beta_2}{(\Sigma\alpha_2)^2}$$

$$E\{(W-\overline{W})^3\} = \frac{48}{m^3(n-1)^2}\frac{\Sigma\alpha_2\beta_2\gamma_2}{(\Sigma\alpha_2)^3} + \frac{8(n-1)(n-2)}{m^3n^4}\frac{\Sigma\alpha_3'\beta_3'}{(\Sigma\alpha_2)^3}$$

$$E\{(W-\overline{W})^4\} = \frac{48}{m^4(n-1)^2}\frac{(\Sigma\alpha_2\beta_2)^2}{(\Sigma\alpha_2)^4} - \frac{96}{m^4(n-1)^2(n+1)}\frac{\Sigma\alpha_2^2\beta_2^2}{(\Sigma\alpha_2)^4}$$
$$+ \frac{72.16}{m^4(n-1)^3}\frac{\Sigma\alpha_2\beta_2\gamma_2\delta_2}{(\Sigma\alpha_2)^4} + \frac{16(n-1)(n-2)(n-3)}{m^4n^5(n+1)}\frac{\Sigma\alpha_4'\beta_4'}{(\Sigma\alpha_2)^4}$$
$$+ \frac{16.12(n-2)}{m^4n^4}\frac{\Sigma\alpha_3'\beta_3'\gamma_2}{(\Sigma\alpha_2)^4} .$$

3. *Comparison of the W- and B-distributions.* Only the first moment of W is independent of the particular numbers $a_1, a_2, \ldots, b_1, b_2, \ldots$. The mean and the variance of the $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution are

$$\frac{1}{m} \quad \text{and} \quad \frac{2(m-1)}{m^2(mn-m+2)}$$

respectively, so that W has always the correct mean value. Its range also is

* The expressions for $E(W)$ and $E\{(W-\overline{W})^2\}$ were given by Welch(3) but not those for the third and fourth moments. The expression K defined in section 3 is equal to Welch's $1-A$.

right, since W must lie between 0 and 1. But its variance is not necessarily correct. If W has approximately a $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution, we must have approximately

$$\frac{4}{m^2(n-1)}\frac{\Sigma\alpha_2\beta_2}{(\Sigma\alpha_2)^2}=\frac{2(m-1)}{m^2(mn-m+2)},$$

that is

$$\frac{2\Sigma\alpha_2\beta_2}{(\Sigma\alpha_2)^2}=\frac{(m-1)(n-1)}{mn-m+2}.$$

Now

$$\frac{2\Sigma\alpha_2\beta_2}{(\Sigma\alpha_2)^2},$$

which we shall denote by K, may have any value between 0 and $(m-1)/m$. It approaches the lower limit when one of the quantities,

$$\alpha_2, \beta_2, \gamma_2, \ldots$$

becomes much larger than all the rest, and it takes the upper limit, $(m-1)/m$, as value when

$$\alpha_2=\beta_2=\gamma_2=\ldots.$$

Hence all that can be said in general about the variance of W is that it is not greater than

$$\frac{2(m-1)}{m^3(n-1)},$$

and that it takes this value when the variance of each batch is the same.

The Eden & Yates experiment(3) was equivalent to taking a sample of a thousand values of W derived from the sets

100	92	0	108
71	0	119	170
197	0	149	161
0	334	140	90
75	43	0	6
0	12	269	337
0	184	71	195
104	100	0	116

Their results showed very good agreement between the W distribution and the $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution, in this case a $B\{1\frac{1}{2}, 10\frac{1}{2}\}$ distribution. For this to be so, the value of K must be approximately

$$\frac{(m-1)(n-1)}{mn-m+2}=\frac{21}{26}=0{\cdot}8077.$$

The batch variances multiplied by four are

7628,	15702,	22669,	59732,
3666,	90593,	26297,	8672,

from which we obtain $K=0{\cdot}7577$,

which is about $\frac{15}{16}$ of the required value. The moments of the two distributions are:

	$B(1\frac{1}{2}, 10\frac{1}{2})$	W
Mean value	0·125	0·125
Variance	0·008413	0·007893
Third moment about mean	0·000901	0·000733
Fourth moment about mean	0·000319	0·000246

From these we should expect that a sample of a thousand values of W would be well fitted by the B-distribution, for the differences between corresponding moments are rather small to be shown up by a sample of a thousand. The standard deviation of the variance of a sample of a thousand values of W is 0·00043, and the W and B variances differ by only about $1\frac{1}{6}$ times this. It is essentially the particular value of K which makes for good agreement.

Usually the terms involving α_3', etc., α_4', etc., will be negligible in comparison with the other terms in the expressions for the moments of W. Assuming that this is so, we have

$$0 \leqq W \leqq 1,$$

$$E(W) = \frac{1}{m},$$

$$E\{(W-\overline{W})^2\} = \frac{2}{m^2(n-1)} \frac{2\Sigma\alpha_2\beta_2}{(\Sigma\alpha_2)^2},$$

and approximately,

$$E\{(W-\overline{W})^3\} = \frac{48}{m^3(n-1)^2} \frac{\Sigma\alpha_2\beta_2\gamma_2}{(\Sigma\alpha_2)^3},$$

$$E\{(W-\overline{W})^4\} = \frac{12}{m^4(n-1)^2} \frac{(2\Sigma\alpha_2\beta_2)^2}{(\Sigma\alpha_2)^4} + \frac{72.16}{m^4(n-1)^3} \frac{\Sigma\alpha_2\beta_2\gamma_2\delta_2}{(\Sigma\alpha_2)^4} - \frac{96}{m^4(n-1)^2(n+1)} \frac{\Sigma\alpha_2^2\beta_2^2}{(\Sigma\alpha_2)^4}.$$

If the value of K is approximately

$$\frac{(m-1)(n-1)}{mn-m+2},$$

the distribution of W will be approximately a $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution, for the range and the mean will be right, the variance will be approximately right, and it will generally be found that the third and fourth moments are approximately right. If a few of the batch variances are very much larger than the rest, the value of K will be too small. In this case there are three alternative procedures. We might discard these batches; if retained without modification they will dominate the experiment. If this is not desirable we might fit a B-distribution by use of the first two moments of W*. The third alternative

* This method has been investigated by Welch in the case of a few uniformity trials ([3] p. 31).

is to make all the batch variances equal by multiplying each batch of numbers by a suitable constant. There seems to be no theoretical objection to this as a preliminary to testing the null hypothesis, and it has the advantage that we then know a great deal more about the distribution of W; but it has the practical disadvantage of involving a lot of calculation. It is mentioned here merely as a theoretical possibility.

If the batch variances α_2, β_2, ... are all equal, or approximately equal, the value of K will be too large, but this will have an appreciable effect only when m and n are both fairly small, for when the batch variances are equal K takes its maximum value,

$$\frac{m-1}{m}, \qquad \text{.....(I)}$$

which, when $m\,(n-1)$ is large, is very close to

$$\frac{(m-1)\,(n-1)}{mn-m+2} = \frac{m-1}{m}\,\frac{1}{1+\dfrac{2}{m\,(n-1)}}, \qquad \text{.....(II)}$$

the value required for the $B\{\frac{1}{2}\,(n-1), \frac{1}{2}\,(m-1)\,(n-1)\}$ distribution. Moreover, K is fairly insensitive to changes in the values of the batch variances when m is large; inequalities in the batch variances will make K less than its maximum value (I) and therefore, provided they are not too great, fairly close to the required value (II).

The tables below show the values of (II) for small values of m and n, and the values of K for various values of the batch variances.

Values of $\dfrac{(m-1)\,(n-1)}{mn-m+2}$

m	n						
	3	4	5	6	7	8	∞
3	0·500	0·545	0·571	0·588	0·600	0·609	0·667
4	0·600	0·643	0·667	0·682	0·692	0·700	0·750
5	0·667	0·706	0·727	0·741	0·750	0·757	0·800
6	0·714	0·750	0·769	0·781	0·789	0·795	0·833

$m=3$

Batch variances	K
1, 1, 1	0·667
1, 1·5, 2	0·642
1, 2, 2	0·640
1, 1, 2	0·625

$m=3$

Batch variances	K
1, 2, 3	0·611
1, 2, 4	0·571
1, 1, 3	0·560
1, 1, 4	0·500

$m=4$

Batch variances	K
1, 1, 1, 1	0·750
1, 1·5, 2, 2·5	0·724
1, 1, 1, 2	0·720
1, 2, 3, 4	0·700
1, 1, 4, 4	0·660
1, 2, 4, 8	0·622

$m=5$

Batch variances	K
1, 1, 1, 1, 1	0·800
1, 3, 3, 3, 5	0·764
1, 2, 3, 4, 5	0·756
1, 1, 4, 6, 6	0·722
1, 1, 1, 4, 4	0·711
1, 1, 2, 4, 8	0·664

Since, in applying the significance test, we require only a rough approximation to the probability of obtaining, if the null hypothesis were true, a value of W not less than that observed, the $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution will very frequently be a sufficiently good approximation to the distribution of W, especially when m and n are large. When either m or n is less than 5, the value of K should be calculated. If this differs considerably from

$$(m-1)(n-1)/(mn-m+2),$$

we can, as suggested above, either fit a B-distribution to the distribution of W by means of the first two moments of W, or equalize the batch variances and use the B-distribution discussed in the next section.

We shall now show that if a B-distribution is fitted by means of the first two moments of W, the third and fourth moments will agree well provided that K is not too small, and hence we may expect a good fit. In other words, if K is not too small, the distribution of W is approximately a B-distribution with mean $1/m$ and variance $2K/\{m^2(n-1)\}$.

From the relation

$$\begin{aligned} 3\Sigma\alpha_2\beta_2\gamma_2 &= \Sigma\alpha_2 . \Sigma\alpha_2\beta_2 - \Sigma\alpha_2^2\beta_2 \\ &= \Sigma\alpha_2 . \Sigma\alpha_2\beta_2 - (\Sigma\alpha_2 . \Sigma\alpha_2^2 - \Sigma\alpha_2^3) \\ &= \Sigma\alpha_2\{3\Sigma\alpha_2\beta_2 - (\Sigma\alpha_2)^2\} + \Sigma\alpha_2^3, \end{aligned}$$

we have

$$\frac{6\Sigma\alpha_2\beta_2\gamma_2}{(\Sigma\alpha_2)^3} = 3K - 2 + \frac{2\Sigma\alpha_2^3}{(\Sigma\alpha_2)^3}.$$

Since the α_2 are all positive,

$$\Sigma\alpha_2\Sigma\alpha_2^3 \geqq (\Sigma\alpha_2^2)^2,$$

and therefore

$$\frac{\Sigma\alpha_2^3}{(\Sigma\alpha_2)^3} \geqq \left\{\frac{\Sigma\alpha_2^2}{(\Sigma\alpha_2)^2}\right\}^2 = (1-K)^2.$$

Thus

$$\frac{6\Sigma\alpha_2\beta_2\gamma_2}{(\Sigma\alpha_2)^3} \geqq 3K - 2 + 2(1-K)^2 = K^2\left(1 - \frac{1-K}{K}\right).$$

Again, $$(\Sigma\alpha_2^2)^3 = \Sigma\alpha_2^6\,(\Sigma\alpha_2^2)^2 > \Sigma\alpha_2^3\Sigma\alpha_2^4 \geqq (\Sigma\alpha_2^3)^2;$$

therefore $$\frac{\Sigma\alpha_2^3}{(\Sigma\alpha_2)^3} < \left\{\frac{\Sigma\alpha_2^2}{(\Sigma\alpha_2)^2}\right\}^{\frac{3}{2}} = (1-K)^{\frac{3}{2}} = (1-K)\,(1-K)^{\frac{1}{2}}$$

$$< (1-K)\,(1-\tfrac{1}{2}K-\tfrac{1}{8}K^2),$$

that is $$< 1-\tfrac{3}{2}K+\tfrac{3}{8}K^2+\tfrac{1}{8}K^3.$$

Hence $$\frac{6\Sigma\alpha_2\beta_2\gamma_2}{(\Sigma\alpha_2)^3} < K^2\frac{3+K}{4}.$$

The third moment about the mean of the B-distribution which has the same first two moments as W is given by

$$\bar{\mu}_3 = \frac{8K^2}{m^3\,(n-1)^2}\,\frac{m-2}{m-1+2K/(n-1)}.$$

The third moment about the mean of the W-distribution is approximately

$$\frac{8K^2}{m^3\,(n-1)^2}\,\frac{6\Sigma\alpha_2\beta_2\gamma_2}{K^2\,(\Sigma\alpha_2)^3}.$$

Since $$\frac{6\Sigma\alpha_2\beta_2\gamma_2}{K^2\,(\Sigma\alpha_2)^3}$$

lies between $$1-\frac{1-K}{K} \text{ and } \frac{3+K}{4},$$

the third moments will be approximately the same provided that K is not too small; for example, with $K=\frac{3}{4}$, $E\,(W-\overline{W})^3$ lies between

$$\frac{2}{3}\,\frac{8K^2}{m^3\,(n-1)^2} \text{ and } \frac{15}{16}\,\frac{8K^2}{m^3\,(n-1)^2}.$$

If $m=5$, $n=4$, the value of $\bar{\mu}_3$ is

$$\frac{2}{3}\,\frac{8K^2}{m^3\,(n-1)^2}.$$

It should be noted that if $K=\frac{3}{4}$, m cannot be less than 4, and that if $m=4$ the batch variances are all the same. Hence

$$E\,(W-\overline{W})^3 = \frac{2}{3}\,\frac{8K^2}{m^3\,(n-1)^2}, \quad \bar{\mu}_3 = \frac{2}{3+3/(2n-2)}\,\frac{8K^2}{m^3\,(n-1)^2}.$$

The fourth moment about the mean of the B-distribution is

$$\frac{12K^2}{m^4\,(n-1)^2}+\frac{48K^3}{m^4\,(n-1)^3},$$

neglecting terms of higher order in $1/m$ and $1/(n-1)$. The first term is the same as the first term in the expression for $E\,(W-\overline{W})^4$. The second term in the expression for $E\,(W-\overline{W})^4$ is

$$\frac{48}{m^4\,(n-1)^3}\,\frac{24\Sigma\alpha_2\beta_2\gamma_2\delta_2}{(\Sigma\alpha_2)^4},$$

which is positive but less than

$$\frac{48}{m^4(n-1)^3}K^2\frac{3+K}{4},$$

since $$24\Sigma\alpha_2\beta_2\gamma_2\delta_2 < \Sigma\alpha_2 . 6\Sigma\alpha_2\beta_2\gamma_2 < (\Sigma\alpha_2)^4 K^2\frac{3+K}{4}.$$

Unless K is very small the third term in the expression for $E(W-\overline{W})^4$ will be negligible in comparison with these, and hence the fourth moments will be about the same.

Without definite knowledge about the batch variances, we cannot discuss further the values of the third and fourth moments of W. It is therefore of some interest to consider the values of these moments for the particular case of equal batch variances, and to see how they agree with the corresponding moments of a B-distribution which has the same first two moments as W. This is a limiting case, and will give us some idea of the truth in other cases, especially when m and n are large. In view of what has already been said this particular case may not be without practical importance. Further, the distribution of W when the batch variances are all equal has a direct practical application in cases where the individuals in a batch are not measured but merely graded or ranked with respect to some character. Our observations then consist of m sets of numbers, each set a permutation of the integers from 1 to n. If the different treatments really produce different effects on the character by which the individuals are ranked, the value of W will tend to be large, for individuals subjected to the same treatment will tend to have about the same rank in each batch. Large values of W are therefore significant. To test for significance, we must know the distribution of W when treatments are ineffective, that is when all possible associations of the rank numbers from the various batches are equally probable.

4. *The approximate distribution of W when the batch variances are equal.*

If $$\alpha_2=\beta_2=\gamma_2=\ldots,$$

we have $$E(W)=\frac{1}{m},$$

$$E\{(W-\overline{W})^2\}=\frac{2(m-1)}{m^3(n-1)}.$$

Assuming, as before, that the terms involving α_3', etc., α_4', etc. are negligible, we have approximately

$$E\{(W-\overline{W})^3\}=\frac{8(m-1)(m-2)}{m^5(n-1)^2},$$

$$E\{(W-\overline{W})^4\}=\frac{12(m-1)^2}{m^6(n-1)^2}+\frac{48(m-1)(m-2)(m-3)}{m^7(n-1)^3}-\frac{48(m-1)}{m^7(n-1)^2(n+1)}.$$

The $B(p, q)$ distribution has the same first two moments as W if

$$p=\frac{n-1}{2}-\frac{1}{m},$$

$$q=\frac{(m-1)(n-1)}{2}-\frac{m-1}{m}.$$

The third moment about the mean of this $B(p, q)$ distribution is

$$\frac{8(m-1)(m-2)}{m^4(n-1)(mn-m+2)}=\frac{8(m-1)(m-2)}{m^5(n-1)^2}\left\{1-\frac{2}{m(n-1)+2}\right\},$$

which agrees well with the corresponding value above when $m(n-1)$ is not too small. The fourth moment about the mean of this $B(p, q)$ distribution is

$$\frac{12(m-1)}{m^6(n-1)^3}\frac{(m-1)y^2+4m^2y-14(m-1)y}{(y+2)(y+4)},$$

where

$$y=m(n-1).$$

The difference between this and the corresponding expression above will be found to be of the same order as the third term in the expression for $E\{(W-\overline{W})^4\}$ and therefore negligible if $m(n-1)$ is not too small. The $B(p, q)$ distribution will thus be a fairly good approximation provided that $m(n-1)$ is not too small.

In order to test the agreement for rather small values of m and n, the following sets of numbers were taken:

−6	−2	3	5
−5	−3	1	7
−3	0	1	4

Numbers proportional to these but with equal batch variances (1/36) are

−0·23250	−0·07750	0·11625	0·19375
−0·18185	−0·10911	0·03637	0·25459
−0·26726	0	0·08909	0·17817

The 576 values of W were calculated, and the following table shows P, the true probability, and P', the probability calculated from the $B(1\frac{1}{6}, 2\frac{1}{3})$ distribution, of obtaining a value of W as great as, or greater than, that shown.

W	P	P'	W	P	P'
0·09	0·894	0·855	0·50	0·253	0·238
0·12	0·797	0·801	0·56	0·188	0·178
0·17	0·691	0·713	0·66	0·099	0·099
0·22	0·595	0·627	0·76	0·050	0·045
0·29	0·493	0·514	0·80	0·026	0·029
0·36	0·396	0·411	0·83	0·019	0·020
0 44	0·297	0·306	0·87	0·010	0·011

For such small values of m and n the agreement is fairly good. It is very good at the upper tail, which is what we are interested in when applying the significance test.

It should be noticed that when m and n are large this $B(p, q)$ distribution will differ very slightly from the $B\{\frac{1}{2}(n-1), \frac{1}{2}(m-1)(n-1)\}$ distribution.

Only the simplest type of analysis of variance test has been discussed in this paper. I had intended another paper to follow, which would deal in the same way with the Latin square arrangement; but this has been dealt with by Welch (5). I may add that Welch's equation (49) on p. 41, giving the variance of W for the Latin square, agrees with my own result, which was reached by a route quite different from his. In view of the rather heavy algebra involved it seems worth while publishing this confirmation of Welch's result.

SUMMARY

The form of the analysis of variance test which involves no assumptions of normality is discussed. Expressions for the first four moments of the statistic used in this test are obtained. From these it appears that when the number of individuals in each batch, and the number of batches are both not too small, the usual test may be safely applied. A method of testing the validity of the approximation which this test employs is stated, and modifications of procedure, when necessary, are suggested.

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ABSENCE AND RECOVERY RECORDED AT IRREGULAR INTERVALS, ILLUSTRATED ON SCHOOL DATA

By FRANK SANDON

CONTENTS

	PAGE
(1) The sickness recovery curve	336
(2) The sickness recovery surface	337
(3) Recording at censuses	340
(4) The case of a day school	341
(5) Sickness recovery as experienced	348
(6) Summary and conclusions	349

(1) The sickness recovery curve

In any population at risk there is a certain morbidity rate, and ideally there would be machinery for recording the number falling sick at any instant, or more precisely, per unit time. Of those who so fall sick, the great majority recover, and ideally that fraction so recovering of the number first recorded would be recorded also at any subsequent instant. If the death-rate from sickness is nil we should then have a sickness recovery curve (s.r.c.) which would approach asymptotically with unlimited time the value zero, the curve giving at any instant the number still sick out of those who fell sick at any specified previous instant.

We may further suppose that of the number who at any particular instant become unfit all are at once as sick then as they will be, i.e. there is no incubation period, and that they all immediately begin, in varying degrees and at varying rates, to recover. The s.r.c. will therefore be a J curve, monotone in its decrease with time. Thus in a particular day secondary school, taking boys and girls of ages 10–19, a table of duration of absence was as follows:

TABLE A

Duration in sessions*	1	2	3	4	5	6	7	8	9	10	11
Frequency	790	403	169	76	111	41	23	31	18	43	11
Duration in sessions	12	13	14	15	16	17	18	19	20	21	22
Frequency	8	13	5	15	5	4	8	1	9	3	0

In addition: 18 cases with duration 23–30 sessions. Total: 1805

* It will be seen below that the concept of "session" as a unit of time is unsatisfactory: this is probably in part the cause of the unusual "humpiness" of the curve.

As is usual with abrupt curves we cannot say whether the curve is very strongly skewed with a mode between 0 and 1 or whether it is a J curve. The

latter as above indicated seems a plausible assumption, and we shall therefore adopt it in the belief that we are not far out in so doing.

(2) THE SICKNESS RECOVERY SURFACE

In any population, however, we should not simply record at any instant the number still sick of those who fell sick at a specific previous instant. We should have the "survivors", i.e. those still sick, of those who fell sick at *all* previous times, and we must now consider this extension of the problem. We may assume as a first approximation that there is a definite rate, in the population, of falling sick, and that this is constant and independent of time. This assumes

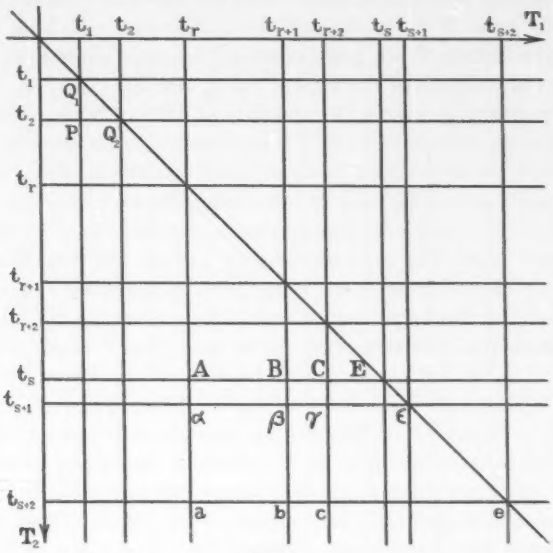

Fig. 1.

that there are no cyclical fluctuations nor epidemics, nor even random variations, a state of affairs that would not be true of any finite real population. If, however, we work on this assumption, we may consider a graphical representation of the number sick at any particular time as obtained by consideration of two perpendicular axes. Consider axes T_1, epoch of falling sick, and T_2, epoch of incapacity. An individual who falls sick at time T_1 is subsequently sick till time T_2 and would be recorded as contributing one unit to the z ordinate until time T_2 when he would disappear from our consideration. We should thus have, for the number still sick, a hollow monotone surface falling away from the line $T_1 = T_2$. Let us consider this in more detail, and for convenience take the T_1 axis as running eastwards for time increasing and the T_2 one as southwards for time increasing (see Fig. 1). This modification of the usual convention of

axes is for convenience in comparing with our tables, where time increasing is indicated both horizontally to the right and vertically downwards. Then the surface itself will have the following features:

(1) It will be bounded on the north-east by an abrupt precipice at $T_1 = T_2$.

(2) The ridge $T_1 = T_2$ running north-west and south-east will be level (assumption of uniform rate of falling sick).

(3) The surface will tend on the south and west from the ridge asymptotically to zero.

(4) Sections of the surface by any line $T_1 = t$ or $T_2 = t$ will be all congruent monotone J curves, with a maximum where the line intersects the ridge. For we see that at any point P, (t_1, t_2), the ordinate is distant $(t_2 - t_1)$ from the maximum ordinate of the section $T_1 = t_1$ and is distant the same amount $(t_2 - t_1)$ from the equal maximum ordinate of the section $T_2 = t_2$ (see Fig. 1, where $PQ_1 = PQ_2$).

In the school already referred to, the data of Table A were obtained from the experience of seven (non-consecutive) terms free from any pronounced epidemic. This experience covered about 250,000 pupil sessions at risk, being approximately 300 pupils on roll for each of 120 sessions for each of seven terms. There were 1805 spells of absence in the experience, totalling 5915 sessions. Absence from whatever cause was included: nearly always this was due to personal sickness. Every case of absence was noted from its first session to its last. Cases that were absent at the beginning or end of term were not considered, nor, for the sake of simplicity, were those very few of more than 3 weeks' duration: seven such are shown in the 1812 cases dealt with in Table I. There was a little difficulty about holidays in the middle of term. These half-term holidays, etc., usually fell on a Saturday or Monday, so that there is some reduction in the number of cases recorded on these days. Some old cases were, however, ascribed to these days for return if the pupil were absent just before the holiday and were back in attendance immediately afterwards: it was assumed that for such cases the pupil's illness was of the modal duration for his appropriate first session of absence. Cases of pupils leaving the school were observed till their last recorded attendance: they then passed from our experience as no longer at risk. The absences were spread over the week as follows:

TABLE B

Day of week	Mon.		Tues.		Wed.	Thurs.		Fri.		Sat.	Total
Session	M.	A.	M.	A.	M.	M.	A.	M.	A.	M.	
Absences on such sessions	550	578	538	600	589	578	606	596	611	669	5915

TABLE I

Sessions in which absences started and finished

Time of last absence		Time of first absence: Mon. M.	Mon. A.	Tues. M.	Tues. A.	Wed. M.	Thurs. M.	Thurs. A.	Fri. M.	Fri. A.	Sat. M.	Total
Mon.	M.	61	—	—	—	—	—	—	—	—	—	61
	A.	127	71	—	—	—	—	—	—	—	—	198
Tues.	M.	11	6	38	—	—	—	—	—	—	—	55
	A.	20	8	67	81	—	—	—	—	—	—	176
Wed.	M.	43	8	44	17	112	—	—	—	—	—	224
Thurs.	M.	1	1	1	2	6	55	—	—	—	—	66
	A.	7	7	12	6	25	83	66	—	—	—	206
Fri.	M.	2	—	3	2	1	7	3	52	—	—	70
	A.	7	—	3	2	3	20	10	73	58	—	176
Sat.	M.	35	5	19	6	18	40	12	46	19	196	396
Mon.	M.	2	1	3	1	1	—	—	4	1	2	15
	A.	5	—	1	—	4	4	2	7	3	15	41
Tues.	M.	—	—	1	—	—	—	—	—	—	1	2
	A.	2	—	1	—	1	1	1	2	1	3	12
Wed.	M.	3	—	1	—	1	1	—	3	—	7	16
Thurs.	M.	—	—	—	—	—	1	—	1	—	—	2
	A.	—	—	—	—	1	1	—	3	1	1	7
Fri.	M.	—	—	—	—	—	—	—	—	—	—	—
	A.	1	—	—	—	—	1	—	—	—	1	3
Sat.	M.	7	—	6	2	3	11	2	9	1	6	47
Mon.	M.	1	—	—	—	—	1	—	—	—	—	2
	A.	—	—	—	—	—	1	1	—	—	2	4
Tues.	M.	—	—	—	—	—	—	—	—	1	—	1
	A.	1	—	—	—	—	—	—	—	—	—	1
Wed.	M.	1	—	—	—	1	2	—	1	—	—	5
Thurs.	M.	—	—	—	—	—	—	—	—	—	1	1
	A.	—	—	—	—	—	—	—	—	—	1	1
Fri.	M.	—	—	—	—	—	—	—	—	—	—	—
	A.	—	—	—	—	—	—	—	—	—	—	—
Sat.	M.	1	—	3	1	1	5	—	2	—	1	14
Mon.	M.	—	—	—	—	—	—	—	1	—	—	1
	A.	—	—	1	—	—	—	—	—	—	—	1
Tues.	M.	—	—	—	—	—	—	—	—	—	—	—
	A.	1	—	—	—	1	—	—	—	—	—	2
Wed.	M.	—	—	—	—	—	—	—	—	—	—	—
Thurs.	M.	—	—	—	—	—	—	—	—	—	—	—
	A.	—	—	—	—	—	—	—	—	—	—	—
Fri.	M.	—	—	—	—	—	—	—	—	—	—	—
	A.	—	—	—	—	—	—	—	—	1	—	1
Sat.	M.	—	1	2	1	—	—	—	1	—	—	5
Total		339	108	206	121	179	234	97	205	86	237	1812

To a first approximation we find that our assumption that the number absent at any census is constant is justified: all the values approximate to the average of 591·5. There is, however, 13% excess on Saturday, whilst the afternoons total 2395 against 2262 for the corresponding mornings, a 6% excess. There is a slight increase during the week, the first four sessions totalling 2266 against 2391 for Thursday and Friday, the increase for the latter again being some 6%. Monday gives 1128, Tuesday 1138 (1% increase), Thursday 1184 (4% increase) and Friday 1207 (2% increase). We shall, however, at this stage in our enquiry neglect these variations in the rate of absence with epoch, remembering only that we shall not expect any very great precision in our results.

(3) Recording at censuses

In actual practice we do not have machinery for recording each case as it falls sick and as it recovers and ceases to be incapacitated, and the S.R. surface cannot be obtained in the manner that we have just outlined. We have instead records, taken at stated times, of individuals who happen at those epochs to be sick. In a population at large we can call these times censuses: the censuses may be roll calls of military units, register markings of schools, clockings-in of factories, or comparable procedures for other communities. As a result, we have records that can be put in the form of Table I. This states that there were x cases that were not sick at time t_r but were sick at time t_{r+1} who had not recovered at time t_s but had recovered at time t_{s+1}. Thus Table I states that there were sixty-one cases who were first absent on a Monday morning and who made that session their last absence, returning in time for the afternoon register marking. Let us consider in our population, in the general case just mentioned, those who had not recovered at time t_s. These will be the "still sick" of those who fell sick at time t_r (represented by the ordinate at the point $A(t_r, t_s)$ (see Fig. 1)), the still sick of those who fell sick just before time t_{r+1} (represented by the ordinate at the point $B(t_{r+1}-\epsilon, t_s)$), and the still sick of those who fell sick at all intermediate times. In other words, those who have not recovered at time t_s are given by an area on the S.R.C. made by the section of the surface by $T_2 = t_s$, the particular area being that lying between $T_1 = t_r$ and $T_1 = t_{r+1}$. The maximum of this S.R.C. will be at E, where $T_1 = t_s$, and the curve will decrease westwards from the ridge (from E towards $BA\ldots$). The number that we have just considered will therefore be given by the area of the portion of an S.R.C. for the length of abscissa AB. Similarly, of those who fell sick between t_r and t_{r+1}, the number who had not recovered by time t_{s+1} will be the area of the S.R.C. for the length $\alpha\beta$. We have therefore

$$x = \text{difference in areas of portions of S.R.C. on } AB \text{ and on } \alpha\beta$$
$$= (\text{area on } AE - \text{area on } BE) - (\text{area on } \alpha\epsilon - \text{area on } \beta\epsilon).$$

Again, y, the number of the same batch falling sick between t_r and t_{r+1} who recover between t_{s+1} and t_{s+2}, is given by

$$y = (\text{area on } \alpha\epsilon - \text{area on } \beta\epsilon) - (\text{area on } ae - \text{area on } be).$$

Hence

$$x + y = (\text{area on } AE - \text{area on } BE) - (\text{area on } ae - \text{area on } be).$$

Similarly, if X and Y are the numbers falling sick between t_{r+1} and t_{r+2} who recover between t_s and t_{s+1} and between t_{s+1} and t_{s+2}, respectively, then

$$X + Y = (\text{area on } BE - \text{area on } CE) - (\text{area on } be - \text{area on } ce).$$

Hence

$$x + y + X + Y = (\text{area on } AE - \text{area on } CE) - (\text{area on } ae - \text{area on } ce).$$

We see thus that the cell values are additive, and that for any four censuses the total can be obtained and will give the difference of areas of the S.R.C. corresponding to the various time intervals.

Suppose that we now have any four of our routine censuses t_1, t_2, t_3, and t_4. We can find the number of individuals who, falling sick between t_1 and t_2, recover between t_3 and t_4. Let this be n. Then n = area on standard S.R.C. between $(t_3 - t_1)$ and $(t_3 - t_2)$ less area on same between $(t_4 - t_1)$ and $(t_4 - t_2)$.

Put $t_1 = -\infty$, and $t_4 = +\infty$, so that A, a and c, if the figure is $ACca$, go to infinity. Then

n = area on standard S.R.C. from C at $(t_3 - t_2)$ to the asymptotic end where the ordinate is zero.

We can check this at once by realizing that we have the whole of the curve except that portion lying between C and E: the ordinates all along ac are now zero. We can in this way build up the area of the S.R.C. For we can consider various values of t_3 and of t_2 in turn. We may also note that, as we suggested in dealing with Table A above, the time element enters in in the form of actual lapse of time and not of sessions.

(4) THE CASE OF A DAY SCHOOL

In the case of the school previously referred to the registers are marked in weekly cycles, ten times each week. Let us take as our unit of time the day, and our zero as midnight Sunday/Monday, and let us take epochs in fractions of the day to the nearest first decimal place. We then have observations at the following times:

TABLE C

Day	Mon.		Tues.		Wed.	Thurs.		Fri.		Sat.
Session	M.	A.	M.	A.	M.	M.	A.	M.	A.	M.
Roll call* at Epoch	9.00 0·4	2.20 0·6	9.00 1·4	2.20 1·6	9.00 2·4	9.00 3·4	2.20 3·6	9.00 4·4	2.20 4·6	9.00 5·4

* It may be noted that the *duration* of a session does not here come into consideration. If a pupil fall sick during the session he may be sent home, or made to lie down, but the register is not amended. If he is fit by the next session in such cases there will then be no record. If he is not fit by the next session, he will then be marked absent for the first time. We note further that any case falling sick between two roll calls and recovering in that interval will not be recorded at all. We assume further that the time taken to come to school is negligible—all cases fit at 9 a.m. are recorded as at school and fit, all cases not fit at 9 a.m. are recorded as absent.

We have seen that the absences can be recorded as in Table I. In this we may consider that the entries repeat themselves in weekly cycles along the top diagonal, so that we can read along this ...61, 71, 38, ..., 58, 196, 61, 71, 38, ..., 58, 196, 61, ... We thus have a table infinitely long along the diagonal, but, as we have already pointed out, giving rise to a sickness recovery surface that will have zero ordinates 21 days away from this diagonal. These zero ordinates will commence therefore along the bottom diagonal, and here the state of affairs is represented in Fig. 2. The surface now lies entirely to the north-east of the diagonal $A_1 A_2 A_3 A_4 A_5$ ... and a_3, for example, could be the one case recorded in Table I as first absent on Tuesday morning (i.e. falling sick in interval epoch $0{\cdot}6d$ to epoch $1{\cdot}4d$) and last absent on the Monday afternoon just short of 3 weeks later (i.e. recovering in interval epoch $21{\cdot}6d$ to $22{\cdot}4d$, where the letter d indicates that the epoch is measured in units of a day). Similarly, b_2 would be the number of cases of the same lot of first absences who recovered between Monday morning roll call (epoch 21·4) and Monday afternoon roll call (epoch 21·6)—in our experience $b_2 = 0$ (see Table I). Then from what we have just seen in the general case c_2, say, is the difference in the areas of sections C_2B_3 and D_2C_3, or what is the same, the difference in the areas of the sections D_2C_2 and C_3B_3. Let us use the notation $|\, AB \,|$ to represent the area of the S.R.C. on the base AB. Then we have the following relations

$$\left.\begin{aligned} |\, A_1B_1 \,| &= a_1 \\ |\, A_2B_2 \,| &= a_2 \\ &\cdots\cdots\cdots\cdots \end{aligned}\right\}, \qquad \text{.....(1·1)}$$

$$\left.\begin{aligned} |\,B_1C_1\,| &= b_1 + |\,A_2B_2\,| \\ |\,B_2C_2\,| &= b_2 + |\,A_3B_3\,| \\ &\cdots\cdots\cdots\cdots \end{aligned}\right\}, \qquad \text{.....(1·2)}$$

$$\left.\begin{aligned} |\,C_1D_1\,| &= c_1 + |\,B_2C_2\,| \\ |\,C_2D_2\,| &= c_2 + |\,B_3C_3\,| \\ &\cdots\cdots\cdots\cdots \end{aligned}\right\}, \qquad \text{.....(1·3)}$$

and so on.

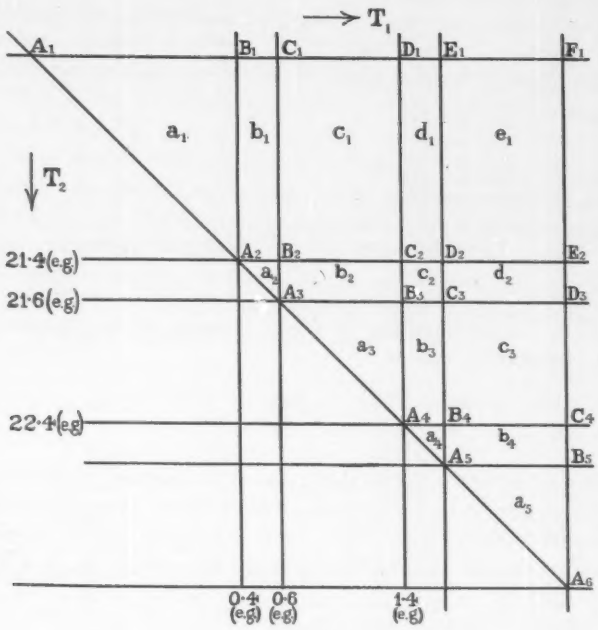

Fig. 2.

Then, adding, we have

$$\left.\begin{aligned} |\,A_1B_1\,| &= a_1 \\ |\,A_1C_1\,| &= a_1 + (a_2 + b_1) \\ |\,A_1D_1\,| &= a_1 + (a_2 + b_1) + (a_3 + b_2 + c_1) \\ |\,A_1E_1\,| &= a_1 + (a_2 + b_1) + (a_3 + b_2 + c_1) + (a_4 + b_3 + c_2 + d_1) \\ &\cdots\cdots\cdots\cdots

Thus Table I gives us

Last absence on	Recovers between	First absence recorded on				
		Mon. m.	Mon. a.	Tues. m.	Tues. a.	Wed.
		Falls sick between				
		$\bar{2}\cdot4$* and 0·4	0·4 and 0·6	0·6 and 1·4	1·4 and 1·6	1·6 and 2·4
Sat. m.	19·4 and 21·4	1	—	3	1	1
Mon. m.	21·4 and 21·6	—	—	—	—	—
Mon. a.	21·6 and 22·4	—	—	1	—	—
Tues. m.	22·4 and 22·6	—	—	—	—	—
Tues. a.	22·6 and 23·4	—	—	—	—	1

* Read bar 2 plus 0·4, i.e. −1·6 before zero epoch.

If we deal with this in the manner just indicated we have, for the curve along $A_1B_1C_1D_1E_1F_1\ldots$ corresponding to a last absence on the Saturday morning referred to, the series of values

21·0	19·0	18·8	18·0	17·8	17·0	...
0	1	1	5	6	8	...

In the same way we can build up the areas on the sections by various values of T_1, remembering always that the entries of the Table I are the difference between the two parallel walls, in either case, of the rectangular slabs. In the case that we have just considered we have, alternatively, by working along $A_6B_5C_4D_3E_2F_1$ in Fig. 2:

21·0	20·2	20·0	19·2	19·0	17·0	...
0	1	1	2	2	8	...

The result 8 can thus be derived in either of two ways, as is obvious from consideration of the formulae (2·1) and the corresponding ones derived for the T_1 sections.

We can thus prepare Table II. In this we note that the results of the series 0, 1, 1, 5, 6, 8 are entered in the Saturday morning column of the first part, and the second series, 0, 1, 1, 2, 2, 8 are the bottom entries of the column Wednesday morning of the second part. We may note that as already pointed out we do not have any readings for triangular portions such as Q_1PQ_2 of Fig. 1, but these are not needed for the computation of the areas of the various sections of the S.R. surface.

The method used for obtaining Table II is of wide generality and can be used at whatever irregular intervals we may take our censuses.

In our case, as we have the decade of roll calls per week, the pattern of entries repeats every 7 days down any column. As, further, our roll calls are all at 9 a.m. (mornings) and 2.20 p.m. (afternoons) the intervals recur at intervals of $0{\cdot}2d$, $0{\cdot}8d$, $1{\cdot}0d$,...etc., and in consequence we have a number of entries (possibly 20 and at least 6), for each interval from the beginning. These entries, for any one interval, should all be equal, but of course in fact, owing to random variations, they differ somewhat. They do however run very closely, in the main, to the same order of magnitude. We note, however, that the T_1 entries increase steadily to the right, owing to the slight increase already reported of "incapacity" during the week, and in particular to the 13% excess on Saturday morning. We shall take the average values of the columns to give the best value for the area of the S.R.C. of the population considered from any abscissa to the tail. Actually we note that these averages show certain irregularities—thus at $14{\cdot}0d$ and at $17{\cdot}0d$ there are increases in the area, whilst on taking differences that from 1·2 to 1·8 is less, over a larger range, than that from 1·0 to 1·2 or from 1·8 to 2·0. We shall not trouble to graduate the result, but proceed immediately to use it to reform Table I, working backwards through the process used to build up Table II. The more important features are given below:

TABLE D

		Mon.		Tues.		Wed.	Thurs.		Fri.		Sat.	Total
		M.	A.	M.	A.	M.	M.	A.	M.	A.	M.	
Cases absent for one session only	Expected	69	55	55	55	139	63	55	55	55	139	740
	Actual	61	71	38	81	112	55	66	52	58	196	790
	$d_1 = A - E.$	−8	+16	−17	+26	−27	−8	+11	−3	+3	+57	+50
Total cases of absence starting on session stated	Expected	332	95	200	95	200	239	95	200	95	200	1751
	Actual	338	107	204	120	179	234	97	204	85	237	1805
	$d_2 = A - E.$	+6	+12	+4	+25	−21	−5	+2	+4	−10	+37	+54
	$d_1 - d_2$	+14	+4	−21	+1	−6	−3	+9	−7	+13	+20	

Again we observe the great excess of Saturday morning cases, many for one session only. It may be noted that this is in spite of special precautions taken at the school in question where this tendency was originally a very pronounced tradition: one method that was found successful with the keener pupils was to

TABLE II

Area of tail of sickness recovery curve from abscissae of specified value. Twenty different estimates

Time interval (days)	Abscissa parallel to T_1 axis										Abscissa parallel to T_2 axis										Av.
	Mon.		Tues.		Wed.	Thurs.		Fri.		Sat.	Mon.		Tues.		Wed.	Thurs.		Fri.		Sat.	
	M.	A.	M.	A.	M.	M.	A.	M.	A.	M.	M.	A.	M.	A.	M.	M.	A.	M.	A.	M.	
0·0	550	578	538	600	589	578	606	596	611	669	550	578	538	600	589	578	606	596	611	669	591·5
0·2	—	471	—	480	—	—	509	—	526	—	471	—	480	—	—	509	—	526	—	—	496·5
0·8	—	—	334	—	410	—	—	392	—	432	—	334	—	410	—	—	392	--	432	—	392·0
1·0	—	—	298	314	371	344	—	361	374	405	298	314	371	—	344	361	374	405	—	—	352·4
1·2	—	—	—	284	—	—	330	—	346	—	284	—	—	—	330	346	—	—	—	—	320·0
1·8	—	—	—	—	272	277	—	—	—	326	—	272	—	277	—	—	326	—	—	—	291·6
2·0	212	—	—	—	250	255	269	265	—	308	250	—	255	269	265	308	—	—	—	212	259·8
2·2	—	194	—	—	—	—	249	—	257	—	—	—	249	—	257	—	—	—	—	194	233·3
2·8	171	—	—	—	—	200	—	229	—	—	—	200	—	229	—	—	—	—	171	—	200·0
3·0	163	155	148	—	—	186	195	215	222	239	186	195	215	222	239	—	—	163	155	148	190·4
3·2	—	148	—	145	—	—	182	—	210	—	182	—	210	—	—	—	—	148	—	145	171·2
3·8	130	—	124	—	—	—	—	173	—	207	—	173	—	207	—	—	130	—	124	—	158·5
4·0	124	119	120	122	131	—	—	167	171	197	167	171	197	—	—	124	119	120	122	131	143·9
4·2	—	113	—	118	—	—	—	—	165	—	165	—	—	—	—	113	—	118	—	—	132·0
4·8	—	—	98	—	111	—	—	—	—	161	—	161	—	—	—	—	98	—	111	—	123·3
5·0	95	—	94	96	108	110	—	—	—	155	155	—	—	—	95	94	96	108	—	110	109·7
5·2	—	84	—	92	—	—	107	—	—	—	—	—	—	—	84	92	—	—	—	107	94·3
5·8	81	—	—	—	88	97	—	—	—	—	—	—	—	81	—	—	88	—	97	—	88·7
6·0	77	71	69	—	85	94	94	99	—	—	—	—	77	71	69	85	—	94	94	99	84·1
6·2	—	68	—	67	—	—	91	—	99	—	—	—	68	—	67	—	—	91	—	99	81·2
6·8	60	—	60	—	—	77	—	87	—	—	—	60	—	60	—	—	77	—	87	—	71·0
7·0	59	54	57	58	61	74	75	85	87	96	59	54	57	58	61	74	75	85	87	96	70·6
7·2	—	54	—	55	—	—	72	—	85	—	54	—	55	—	—	72	—	85	—	—	66·5
7·8	—	—	44	—	53	—	—	72	—	85	—	44	—	53	—	—	72	—	85	—	63·5
8·0	—	—	44	43	50	51	—	69	72	83	44	43	50	—	51	69	72	83	—	—	58·9
8·2	—	—	—	43	—	—	50	—	69	—	43	—	—	—	50	69	—	—	—	—	54·0
8·8	—	—	—	—	39	44	—	—	—	70	—	39	—	44	—	—	70	—	—	—	51·0

TABLE II (*continued*)

9·0	35	—	—	—	39	41	43	48	—	67	39	—	41	43	48	67	—	—	—	35	45·5
9·2	—	32	—	—	—	—	40	—	48	—	—	—	40	—	48	—	—	—	—	32	40·0
9·8	30	—	—	—	—	31	—	42	—	—	—	31	—	42	—	—	—	—	30	—	34·3
10·0	29	27	27	—	—	31	30	39	42	47	31	30	39	42	47	—	—	29	27	27	34·0
10·2	—	26	—	26	—	—	30	—	39	—	30	—	39	—	—	—	25	26	—	26	30·2
10·8	25	—	24	—	—	—	—	29	—	41	—	29	—	41	—	24	22	—	24	—	29·7
11·0	24	22	23	23	24	—	—	29	29	38	29	29	38	—	—	21	—	23	23	24	26·5
11·2	—	21	—	23	—	—	—	—	29	—	29	—	—	—	—	—	19	—	—	—	24·3
11·8	—	—	19	—	21	—	—	—	—	28	—	28	—	—	15	19	19	21	21	—	22·7
12·0	15	—	19	19	21	19	—	—	—	28	28	—	—	—	13	19	—	—	—	19	20·0
12·2	—	13	—	19	—	—	18	—	—	—	—	—	—	—	—	—	17	—	16	18	16·7
12·8	12	—	—	—	17	16	—	17	—	—	—	—	11	12	12	17	—	16	16	—	15·0
13·0	11	10	12	—	17	16	16	—	17	—	—	—	9	10	12	—	13	16	—	17	14·1
13·2	—	9	—	12	—	—	16	—	—	—	—	7	—	9	—	13	13	—	16	17	13·5
13·8	7	—	9	—	—	13	—	16	—	—	7	5	8	9	10	13	—	16	16	—	11·2
14·0	7	5	8	9	10	13	13	16	16	17	5	—	8	—	—	13	13	16	—	17	11·4
14·2	—	5	—	8	—	—	13	—	16	—	—	4	—	7	8	—	—	—	16	—	10·5
14·8	—	—	4	—	7	—	—	13	—	16	4	4	6	—	—	13	13	16	—	—	10·0
15·0	—	—	4	4	6	8	—	13	13	16	4	—	—	—	8	13	—	—	—	—	9·1
15·2	—	—	—	4	—	—	8	—	13	—	4	—	—	6	8	—	13	—	—	—	8·3
15·8	—	—	—	—	2	6	—	—	—	13	—	2	—	6	8	13	—	—	—	3	7·0
16·0	3	—	—	—	2	5	6	8	—	13	2	—	5	6	8	—	—	—	—	2	6·2
16·2	—	2	—	—	—	—	5	—	8	—	—	—	5	—	8	—	—	—	3	—	5·0
16·8	3	—	—	—	—	1	—	6	—	—	—	1	—	6	—	—	—	3	2	1	3·3
17·0	3	2	1	—	—	1	1	5	6	8	1	1	5	6	8	—	—	2	—	1	3·4
17·2	—	2	—	1	—	—	1	—	5	—	1	—	5	—	—	—	2	—	1	—	2·2
17·8	2	—	1	—	—	—	—	1	—	6	1	1	—	6	—	2	2	1	1	0	2·5
18·0	2	2	1	1	0	—	—	1	1	5	1	—	5	—	—	2	—	1	—	—	1·6
18·2	—	2	—	1	—	—	—	—	1	—	1	—	—	—	—	—	1	—	0	—	1·3
18·8	—	—	1	—	0	—	—	—	—	1	—	1	—	—	2	1	1	0	—	0	0·7
19·0	2	—	1	1	0	0	—	—	—	—	1	—	—	—	2	1	—	—	0	0	0·8
19·2	—	2	—	1	—	—	0	—	—	—	—	—	—	1	—	—	0	0	—	0	1·0
19·8	1	—	—	—	0	0	—	—	—	—	—	—	1	1	1	0	—	0	0	0	0·3
20·0	1	1	1	—	0	0	0	0	—	—	—	—	1	—	1	—	—	0	—	0	0·4
20·2	—	1	—	1	—	—	0	—	0	—	—	—	—	0	—	—	0	—	0	0	0·5
20·8	0	—	0	—	—	0	—	0	—	—	—	0	—	—	—	—	0	—	0	—	0·0
21·0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0·0

hold the headmaster's terminal examinations week by week on Saturdays. We note further the tendency, again more pronounced for the one session cases, for absence in the afternoons. I fancy that the reason is that if Tommy feels a slight malaise in the afternoon his mother (it is the mother who settles these matters) says, "Well, it's only three lessons this afternoon and one of them is P.T. and one is Art. He needn't go." The same malaise would not be enough to keep him at home for the longer and harder morning session. It would be of interest to note if this feature of afternoon absences were more in evidence in schools with a very short afternoon session than in the one of our experience (mornings 3 hr. 20 min., afternoons 2 hr. 10 min.).

On the other hand, we note that there are a number who attend on Wednesdays who apparently would not come that session if it began a full day: the attitude may be: "Well, I'll see it out this morning, and can go to bed this afternoon if I am not better then." This introduces a practical consideration in organization, for in general the more nearly equal we make our census intervals the greater is the total absence recorded (this follows as an easy corollary from the assumption of a monotone hollow decreasing S.R.C.) so that a 6 morning-4 afternoon week would in the ordinary way give more absences than a 5 morning-5 afternoon week. If however Wednesday were a full day, then perhaps some of those who now attend on Wednesday would fail to do so. There may be of course some countervailing effect on the attendance for the last session of the week. I am not aware of any data comparable to ours for a 5-day week school. It is relevant to note here that administrators have expressed some concern at the psychological effects (in excessive "week-ending", etc.) of the 5-day week (see, e.g., J. S. Hart, *J.S.S.* vol. LXXXV, pt III, pp. 349–411, May 1922). We should point out that in none of the terms of our experience were there more than two Jews (observing Saturday sabbath) in the school and for most of the time there was none.

(5) Sickness recovery as experienced

By differencing the areas of the last column of Table II we could obtain the ordinates of the S.R.C. As, however, the areas are not smoothed the errors will be, as already pointed out, noticeable at an early stage, and no great reliance can be put on the results. The differences for important bands of width $0{\cdot}2d$ are given by the following summary:

TABLE E

Beginning at...d	0·0	1·0	2·0	3·0	4·0	7·0	14·0	21·0
Difference	95·0	32·4	[illegible]	19·2	11·9	4·1	0·9	0·0
$l_x = 100$	100	34	[illegible]	20	12·5	4	1	0

We can conclude that recovery for these adolescent boys and girls is very rapid: two-thirds of them are fit again in 24 hr., three-quarters in 2 days, and only 1 % will be more than a fortnight absent.

(6) SUMMARY AND CONCLUSIONS

1. From observations at irregular intervals of the numbers of a population absent, a method is given for obtaining the curve of recovery from sickness (or whatever the cause of absence may be).

2. The sickness recovery curve is a monotone curve, and the total of absences is least for a definite number of censuses in any time if the censuses are equally spaced.

3. The observations of numbers absent and recovering at various censuses give portions of the area of the S.R.C.: we do not know and need not have the number who both fall sick and recover in any intercensal interval.

4. The method is applied to a secondary school population of about a quarter of a million pupil sessions at risk, and the actual and predicted absences compared.

5. Some administrative aspects of the intervals between register markings are pointed out.

6. For the particular adolescent boys and girls, the conclusion is reached that two-thirds of them are fit within one day, and that generally recovery is very rapid.

THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN TWO MEANS WHEN THE POPULATION VARIANCES ARE UNEQUAL

By B. L. WELCH, Ph.D.

1. *Introduction.* Suppose that we have samples of sizes n_1 and n_2 from populations π_1 and π_2 respectively. Let the populations be normal in form, π_1 having mean and standard deviation α_1 and σ_1, and π_2 having mean and standard deviation α_2 and σ_2. Let it be required to test whether $\alpha_1 = \alpha_2$. Two cases may be distinguished: (i) σ_1 and σ_2 may be equal or (ii) they may be unequal. In the first case the most appropriate test for the equality of the α's is made by referring the criterion

$$u = \frac{(\bar{x}_1 - \bar{x}_2)}{\sqrt{\frac{\Sigma_1 + \Sigma_2}{(n_1 + n_2 - 2)}\left(\frac{1}{n_1} + \frac{1}{n_2}\right)}} \qquad \ldots\ldots(1)$$

to the t distribution with $f = (n_1 + n_2 - 2)$.* In the second case, if the ratio of the two σ's is known, a similar criterion can be used: if, however, this ratio is unknown, no criterion quite so simple is available. A solution of the problem of testing the hypothesis in this instance has been proposed by R. A. Fisher,† using the concept of fiducial distributions. Fisher notes the equivalence of his test to that given previously by W. V. Behrens‡ in 1929. The validity of this test has, however, been questioned by M. S. Bartlett.§ An alternative‖ criterion which has been often employed is

$$v = \frac{(\bar{x}_1 - \bar{x}_2)}{\sqrt{\frac{\Sigma_1}{n_1(n_1 - 1)} + \frac{\Sigma_2}{n_2(n_2 - 1)}}}. \qquad \ldots\ldots(2)$$

This may be referred to the normal probability table if the samples are large enough, but for small samples it does not yield an exact test and it is not clear how it may best be made to furnish approximations.

It has been pointed out by Fisher that in many practical situations where u is used, the fact that the σ's must be equal for the criterion to be distributed as t does not necessarily mean that an *assumption* of equality is involved. It may

* Σ_1 denotes the sum of squares of the observations in the first sample from their mean. Σ_2 similarly for the second sample.

† *Ann. Eugen.* VI, Part IV (1936), p. 396.

‡ *Landw. Jb.* LXVIII (1929), p. 822.

§ M. S. Bartlett. *Proc. Camb. Phil. Soc.* XXXII, Part 4 (1936), p. 564.

‖ It should be noted that, if $n_1 = n_2$, the criteria u and v are identical.

mean that the equality of the σ's is being regarded as part of the hypothesis under test. In such situations it may be argued that there is no point in testing whether $\alpha_1 = \alpha_2$ unless we have also $\sigma_1 = \sigma_2$. However, even if the question posed is one of testing whether two normal populations are identical, u will not necessarily be the best criterion to use. u will afford a valid* test, in the sense that it will control satisfactorily the chance of rejecting the hypothesis when it is actually true, but it is only one of many such. The choice of criterion must depend on what sort of departure from the hypothesis under test we are most interested in detecting. u is demonstrably the best criterion when we wish to detect differences in means without attendant differences in standard deviations. It is conceivable, however, that the test based on u may sometimes operate in such a fashion that differences in the standard deviations σ_1 and σ_2 may mask differences in the means α_1 and α_2, with the result that judgments of non-significance may be too frequently made. The investigations in this paper throw some light on this point, although explicitly they are concerned with cases where it is reasonable to test whether $\alpha_1 = \alpha_2$, whatever the ratio of σ_1 to σ_2.

In the first place I shall consider the problem—how far is the criterion u valid even when $\sigma_1 \neq \sigma_2$? (That the test is liable to be biased in this instance is generally realized, but the extent of the bias has not hitherto received any detailed discussion.) In the second place I shall consider the validity of testing the hypothesis by referring v to the t distribution with $f = (n_1 + n_2 - 2)$. Finally, I wish to make some observations about the test of Fisher and Behrens, mentioned above.

It is easily seen that u in general is not distributed as t. For whereas the square of the standard error of $(\bar{x}_1 - \bar{x}_2)$ is $(\sigma_1^2/n_1 + \sigma_2^2/n_2)$, the quantity under the root in (1) is an unbiased estimate of

$$\frac{(n_1 - 1)\sigma_1^2 + (n_2 - 1)\sigma_2^2}{(n_1 + n_2 - 2)}\left(\frac{1}{n_1} + \frac{1}{n_2}\right).$$

This is equal to $(\sigma_1^2/n_1 + \sigma_2^2/n_2)$ only if $\sigma_1 = \sigma_2$ or $n_1 = n_2$. The criterion v does not suffer from this objection, but its distribution still depends to a certain extent on σ_1/σ_2. The first problem will be to obtain the distributions of u and v. The exact distributions will not be derived here, but only certain approximations adequate, I believe, for the purpose in hand.

2. *The distributions of u and v.* When $\alpha_1 = \alpha_2$ we may write

$$(\bar{x}_1 - \bar{x}_2) = \chi' \sqrt{\sigma_1^2/n_1 + \sigma_2^2/n_2}; \quad \Sigma_1 = \chi_1^2 \sigma_1^2; \quad \Sigma_2 = \chi_2^2 \sigma_2^2, \qquad \ldots\ldots(3)$$

where χ'^2, χ_1^2 and χ_2^2 are independently distributed as χ^2 with degrees of freedom 1,

¶ The term "validity" applied to a test is used throughout this paper in the sense here indicated The term "unbiased" is also used with the same meaning, which should not be confused with the meaning which J. Neyman and E. S. Pearson have attached to it in recent papers on testing statistical hypotheses.

(n_1-1) and (n_2-1) respectively. It is therefore possible to write both u and v in the form

$$y=\frac{\chi'}{\sqrt{a\chi_1^2+b\chi_2^2}}=\frac{\chi'}{\sqrt{w}} \text{ (say)}, \qquad \ldots\ldots(4)$$

where a and b are constants depending on the n's and σ's. w is always distributed independently of χ' and, when $a=b$ or when either a or b is zero, w is distributed as χ^2 multiplied by some constant. In these cases y will be distributed as t multiplied by some constant. For other values of a and b the distribution is not so simple, but a useful approximation may be obtained. Following the lines adopted in a previous paper* let us first approximate to the distribution of w by the Pearsonian Type III Curve

$$p(w)=\frac{1}{(2g)^{\frac{1}{2}f}\Gamma(\frac{1}{2}f)}w^{\frac{1}{2}f-1}e^{-\frac{w}{2g}}, \qquad \ldots\ldots(5)$$

where f and g are so chosen that the first two moments of the curve agree with the true moments of w. For the curve we have

$$\text{mean}=gf; \quad \mu_2=2g^2f$$

and for the true moments of w

$$\text{mean}=(af_1+bf_2); \quad \mu_2=2(a^2f_1+b^2f_2),$$

f_1 and f_2 now being written instead of (n_1-1) and (n_2-1). Hence

$$g=\frac{a^2f_1+b^2f_2}{af_1+bf_2}; \quad f=\frac{(af_1+bf_2)^2}{a^2f_1+b^2f_2}. \qquad \ldots\ldots(6)$$

With these values of f and g we see from (5) that w/g is distributed approximately as χ^2 with f degrees of freedom. Hence χ' divided by $\sqrt{w/fg}$ is distributed approximately as t. Therefore from (4) we have $y=ct_f$, where

$$c=\frac{1}{\sqrt{fg}}=\frac{1}{\sqrt{af_1+bf_2}}, \qquad \ldots\ldots(7)$$

and t_f is distributed approximately as t with degrees of freedom f† given by (6). This approximation is sufficiently close for the purpose of the comparisons made in this paper‡ and it will be used throughout. The term "approximation" will be omitted.

From (1) and (3) it is seen that u is of the form (4), where

$$a=\frac{\sigma_1^2\left(\frac{1}{n_1}+\frac{1}{n_2}\right)}{(n_1+n_2-2)\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)}; \quad b=\frac{\sigma_2^2\left(\frac{1}{n_1}+\frac{1}{n_2}\right)}{(n_1+n_2-2)\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)}.$$

* B. L. Welch. *J. Roy. Statist. Soc.* Supplement III, No. 1 (1936). p. 47.

† f is, of course, not now necessarily an integral number of degrees of freedom. It is simply a term in a mathematical approximation. This approximation is of the same form as a true t distribution and hence we may regard f as effectively a number of degrees of freedom.

‡ For some further discussion of the adequacy of this approximation, see a Note by Miss E. Tanburn at the end of this paper.

Hence from (6) and (7) we have $u = ct_f$, where

$$f = \frac{\{(n_1-1)\sigma_1^2 + (n_2-1)\sigma_2^2\}^2}{\{(n_1-1)\sigma_1^4 + (n_2-1)\sigma_2^4\}}; \quad c = \sqrt{\frac{(n_1+n_2-2)\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)}{\left(\frac{1}{n_1}+\frac{1}{n_2}\right)\{(n_1-1)\sigma_1^2 + (n_2-1)\sigma_2^2\}}}. \quad \text{...(8)}$$

Similarly for v we have

$$a = \frac{\frac{\sigma_1^2}{n_1(n_1-1)}}{\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)}; \quad b = \frac{\frac{\sigma_2^2}{n_2(n_2-1)}}{\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)},$$

and we can write $v = ct_f$, where

$$f = \frac{\left(\frac{\sigma_1^2}{n_1}+\frac{\sigma_2^2}{n_2}\right)^2}{\frac{\sigma_1^4}{n_1^2(n_1-1)}+\frac{\sigma_2^4}{n_2^2(n_2-1)}}; \quad c = 1. \qquad \text{.....(9)}$$

3. *The validity of the criterion u.* Suppose that u is being used to test the hypothesis that $\alpha_1 = \alpha_2$ and that the risk of rejecting the hypothesis when true is to be fixed at some prescribed level ϵ. If it can be assumed that $\sigma_1 = \sigma_2$, then from t tables with (n_1+n_2-2) degrees of freedom it is possible to choose u_0 such that the chance $P(|u| > u_0) = \epsilon$. If u_0 is so chosen, but it happens that $\sigma_1 \neq \sigma_2$, then the test, which consists in rejecting the hypothesis when $|u| > u_0$, will be biased. We shall have

$$P(|u| > u_0) = P(|ct_f| > u_0) = P\left(|t_f| > \frac{u_0}{c}\right), \qquad \text{.....(10)}$$

where c and f are given by (8). Owing to the connection between the t distribution and the Beta-function it can be shown that

$$P(|t_f| > t_0) = I_{\frac{f}{f+t_0^2}}\left(\tfrac{1}{2}f, \tfrac{1}{2}\right),$$

where

$$I_{z_0}(p,q) = \frac{1}{B(p,q)}\int_0^{z_0} z^{p-1}(1-z)^{q-1}dz.$$

Hence from (10)

$$P(|u| > u_0) = I_{z_0}\left(\tfrac{1}{2}f, \tfrac{1}{2}\right), \qquad \text{.....(11)}$$

where

$$z_0 = \frac{f}{\left(f+\frac{u_0^2}{c^2}\right)}. \qquad \text{.....(12)}$$

Since for given sample sizes, c and f depend only on the ratio $\theta = \sigma_1^2/\sigma_2^2$, it is possible from (11) and (12) to obtain for any θ the chance of rejection of the hypothesis when it is true. This dependence on θ is best illustrated by taking particular examples.

Example I. Let $n_1 = n_2 = 10$. Suppose the chance of rejection ϵ is to be fixed at 0·05. The value of u_0 appropriate when $\theta = 1$ is found to be 2·101. In this case, as always when the n's are equal, c is unity. f is $9(\theta+1)^2/(\theta^2+1)$. The values of $P(|u| > 2{\cdot}101)$ for different θ were obtained from (11), using the Incomplete Beta-function Tables,* and are plotted in Fig. 1 as curve (a). For convenience θ is on a logarithmic scale. It is seen that P always lies between 0·05 and 0·065, the latter value being attained when the variation in one of the populations is zero. The test is therefore never very seriously biased.

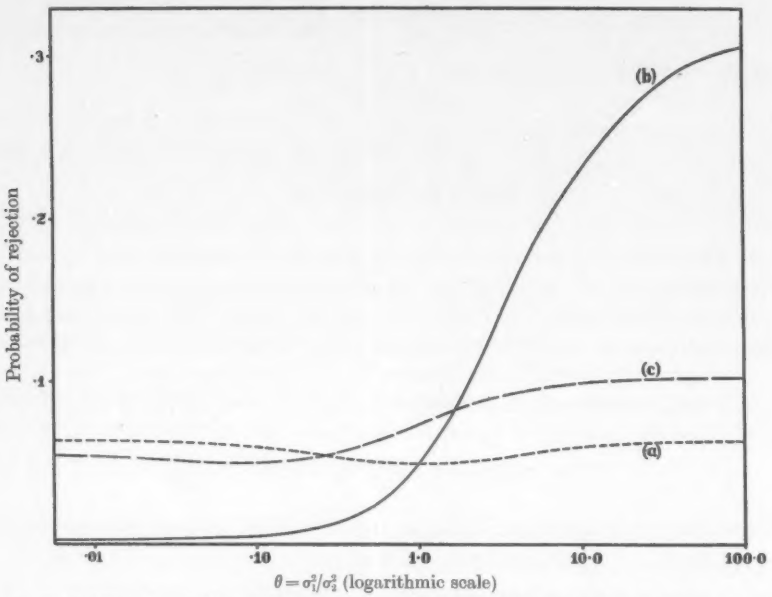

Fig. 1. Probability of rejection of hypothesis $\alpha_1 = \alpha_2$ when true plotted against θ. (a) $n_1 = n_2 = 10$, $P(|u| > 2{\cdot}101)$; (b) $n_1 = 5$, $n_2 = 15$, $P(|u| > 2{\cdot}101)$; (c) $n_1 = 5$, $n_2 = 15$, $P(|v| > 2{\cdot}101)$.

Example II. Let $n_1 = 5, n_2 = 15, \epsilon = 0{\cdot}05$. In this case $(n_1 + n_2 - 2)$ is 18 as before and $u_0 = 2{\cdot}101$. (8) gives

$$f = \frac{(4\theta+14)^2}{(4\theta^2+14)}; \quad c^2 = \frac{18(3\theta+1)}{4(4\theta+14)}.$$

$P(|u| > 2{\cdot}101)$ is plotted against θ in Fig. 1 (b). It will now be seen that P varies from 0·0024 when $\theta = 0$ to 0·05 when $\theta = 1$ and then to 0·313 when $\theta = \infty$. There is therefore the possibility of a considerable bias in the test. The significance of the difference between the two means will tend to be under-estimated when $\sigma_1 < \sigma_2$

* *Tables of the Incomplete Beta-function*, edited by Karl Pearson, Biometrika Office, University College, London (1934).

and overestimated when $\sigma_1 > \sigma_2$. The reason for this is not so much that f may differ from 18, but that c can differ considerably from unity. In general the greater the disparity between the n's the more likely is this c factor to bias the test. For equal-sized samples, except perhaps when they are as small as two, the test is never very much biased, whatever θ.

4. *The validity of the criterion v.* The validity of the procedure of testing the hypothesis by referring the criterion v to the t distribution with (n_1+n_2-2) degrees of freedom may be investigated in the same manner. When the n's are equal there is no need for separate discussion as u and v are then identical. Let us consider the case $n_1=5$, $n_2=15$. We find

$$f=\frac{28\,(3\theta+1)^2}{(63\theta^2+2)}; \quad c=1.$$

$P(|v|>2{\cdot}101)$, obtained from an equation similar to (11), is plotted against θ in Fig. 1 (*c*). As θ increases from 0 to 2/21 P decreases from 0·054 to 0·050 and then increases again to 0·104 at $\theta=\infty$. It is seen that the test formulated in this way is only unbiased when $\theta=2/21$. There is not, however, the possibility of so large a bias as occurs for some values of θ when using the u criterion. The reason of course is that c is always unity, this being guaranteed by the fact that the expectation of the square of the denominator of v is $(\sigma_1^2/n_1+\sigma_2^2/n_2)$. Bias is due solely to f being in general less than 18. When $\theta=0$, f is only 14. f increases to 18 at $\theta=2/21$ and then decreases to 4 at $\theta=\infty$. When the smaller sample comes from the more variable population the effective number of degrees of freedom f is liable to be much smaller than (n_1+n_2-2). Even when $\theta=1$ the effective degrees of freedom in the present case are 6·89, as against 18 for u. If it is known that $\sigma_1=\sigma_2$, then there can be no doubt that u is a better, more sensitive,* criterion than v. If, however, there exists the possibility that σ_1 and σ_2 differ, then u may give very misleading results and it will be safer to use v.†

5. *The comparison of regressions.* It has been found that a criterion based on an estimate of an assumed common variance may lead to a biased test if n_1 and n_2 are different. In the majority of cases it is possible to arrange that n_1 and n_2 are equal or almost so, and hence, practically, serious errors of the kind discussed in the previous sections will not often occur. But there is a more general class of problem where the assumption of equal variances may lead to trouble. An instance is afforded by the test for the equality of linear regression coefficients. Here the usual criterion is

$$u=\frac{(b_1-b_2)}{\sqrt{\dfrac{(\Sigma_1+\Sigma_2)}{(n_1+n_2-4)}\left(\dfrac{1}{\Sigma\,(x-\bar{x}_1)^2}+\dfrac{1}{\Sigma\,(x-\bar{x}_2)^2}\right)}},$$

* For further discussion of what is meant here by "sensitivity", see § 6.

† Fig. 1 clearly shows that θ need not differ much from unity before v becomes less biased than u. (This refers of course to the particular sample sizes $n_1=5$, $n_2=15$.)

where b_1 and b_2 are sample regressions and Σ_1 and Σ_2 are now sums of squares from the fitted regression straight lines. Considerations similar to the above show that even if the sample sizes are equal, unless $\Sigma(x-\bar{x}_1)^2$ and $\Sigma(x-\bar{x}_2)^2$ are also equal, the test is biased when the residual variances about the two population regression lines are not the same.

More generally suppose that we have a situation where the samples yield independent statistics T_1, Σ_1, T_2, Σ_2. Let T_1 be normally distributed about α_1 with standard deviation $\sqrt{V_1}\sigma_1$ and Σ_1 be distributed as $\chi_1^2\sigma_1^2$ with f_1 degrees of freedom. Let T_2 be normally distributed about α_2 with standard deviation $\sqrt{V_2}\sigma_2$ and Σ_2 be distributed as $\chi_2^2\sigma_2^2$ with f_2 degrees of freedom. To test whether $\alpha_1=\alpha_2$ we may use the criterion

$$u=\frac{(T_1-T_2)}{\sqrt{\dfrac{(\Sigma_1+\Sigma_2)(V_1+V_2)}{(f_1+f_2)}}}.$$

This is appropriate if $\theta=1$. Otherwise u is approximately distributed as ct_f, where

$$f=\frac{(f_1\theta+f_2)^2}{(f_1\theta^2+f_2)};\quad c^2=\frac{(f_1+f_2)(V_1\theta+V_2)}{(f_1\theta+f_2)(V_1+V_2)}.$$

The effective number of degrees of freedom is (f_1+f_2) only when $\theta=1$. The chief cause of bias, however, is likely to arise from the c^2 factor. When $\theta=0$, c^2 is $V_2(f_1+f_2)/f_2(V_1+V_2)$ and when $\theta=\infty$, c^2 is $V_1(f_1+f_2)/f_1(V_1+V_2)$. c^2 increases or decreases steadily between these limits according as V_1/f_1 is greater or less than V_2/f_2. c^2 is only uniformly equal to unity if V_1 and V_2 are inversely proportional to f_1 and f_2. In the simple regression case f_1 and f_2 are (n_1-2) and (n_2-2). V_1 and V_2 are $1/\Sigma(x-\bar{x}_1)^2$ and $1/\Sigma(x-\bar{x}_2)^2$. When $n_1=n_2$, c^2 is uniformly unity for all θ only if $\Sigma(x-\bar{x}_1)^2=\Sigma(x-\bar{x}_2)^2$.

The alternative criterion which makes use of separate estimates of σ_1^2 and σ_2^2 is

$$v=\frac{(T_1-T_2)}{\sqrt{\dfrac{V_1\Sigma_1}{f_1}+\dfrac{V_2\Sigma_2}{f_2}}}.$$

This leads to

$$f=\frac{(V_1\theta+V_2)^2}{\left(\dfrac{V_1^2\theta^2}{f_1}+\dfrac{V_2^2}{f_2}\right)};\quad c=1. \qquad \text{.....(13)}$$

Any bias is now due only to the effective number of degrees of freedom which is never less than the smaller of f_1 and f_2. It is clear that in certain situations where a criterion of the u type is customarily used, the condition $\theta=1$ needs to be satisfied very stringently. A criterion of the v type will be much safer.

6. *Choice of effective number of degrees of freedom for v.* The question remains—if v is used, what is the best value to take for f? In the above discussion the

consequences of referring v to t tables with (f_1+f_2) degrees have been considered. It was seen that this was absolutely valid only if θ had a particular value. (In the example of Fig. 1 this was $\theta=2/21$. In general it is $\theta=V_2f_1/V_1f_2$.) When there is strong *a priori* reason for believing that θ is in the neighbourhood of a certain value, then it will be better to take the f obtained by substituting this value in (13). For instance suppose that we have reason to believe that $\theta \simeq 1$. Then, for the v test, it will be preferable to take

$$f=\frac{(V_1+V_2)^2}{\left(\frac{V_1^2}{f_1}+\frac{V_2^2}{f_2}\right)}. \qquad \text{.....(14)}$$

In the example of section 4 this gives $f=6{\cdot}89$ and the corresponding critical value v_0 is 2·374. In Fig. 2 a comparison is made for different θ between the two rules:

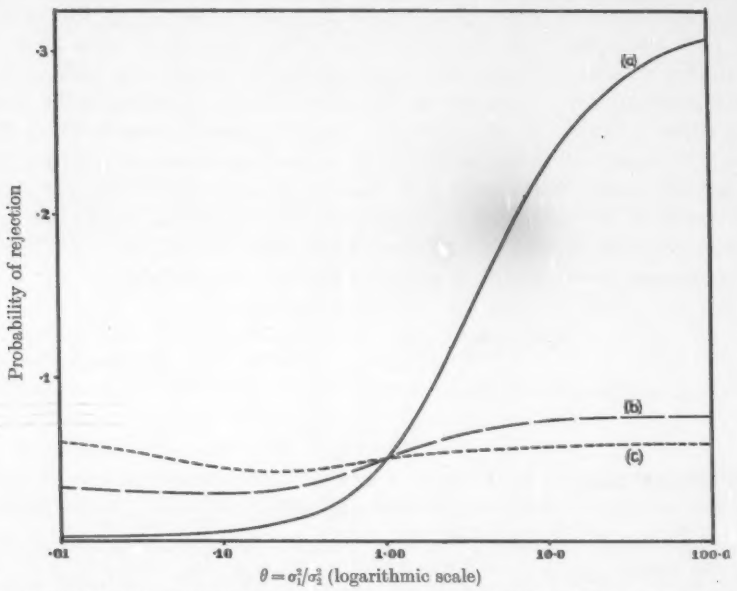

Fig. 2. Probability of rejection of hypothesis $\alpha_1=\alpha_2$ when true plotted against θ. (*a*) $n_1=5$, $n_2=15$, $P(|u|>2{\cdot}101)$; (*b*) $n_1=5$, $n_2=15$, $P(|v|>2{\cdot}374)$; (*c*) $n_1=5$, $n_2=15$, $P(|z|>1{\cdot}861)$.

(*a*) reject the hypothesis that $\alpha_1=\alpha_2$ if $|u|>2{\cdot}101$ and (*b*) reject if $|v|>2{\cdot}374$. As arranged, now, both these rules have the property that for $\theta=1$, the chance of rejection of the hypothesis when true is 0·05.

It may be objected that it is illogical to use the v criterion and at the same time regard it as having effective degrees of freedom f given by substituting $\theta=1$ in (13). For, if $\theta=1$, it is known that the u test is better from the point of view of sensitivity, i.e. any real difference $(\alpha_1-\alpha_2)$ will then be detected more frequently

by u than by v. However in taking the value of f given by (14) we are not assuming that θ is *exactly* unity, but simply making use of our reasons for believing that it is near unity. The v test based on this value of f is biased when $\theta \neq 1$ but the bias is seen to be less than that of the u test. The small* advantage which u enjoys with respect to sensitivity when $\theta = 1$ is soon offset by this gain of v in controlling the chance of rejecting the hypothesis that $\alpha_1 = \alpha_2$ when it is actually true.

When there is no very precise *a priori* information about θ available it might seem permissible to use the ratio Σ_1/Σ_2 from the samples to estimate θ and hence f. Complications arise, however, owing to the fact that the distribution of v is not independent of that of Σ_1/Σ_2. A discussion of this point is beyond the scope of this paper.

7. *The fiducial test of R. A. Fisher.* I have so far considered only two of many criteria which may be proposed to test the hypothesis that $\alpha_1 = \alpha_2$. I have moreover been concerned with one aspect only of the tests based upon these two criteria, viz. whether they control satisfactorily the risk of rejecting the hypothesis when it is actually true. A test which does control this risk is termed in the present paper either a "valid" or an "unbiased" test. The special sense in which these terms have been used should be noted, for a valid test is not necessarily a good test nor vice versa. In the present section I propose to discuss the fiducial test suggested by R. A. Fisher from the single point of view only of how far it is valid, in the sense defined above, for all values of the ratio $\theta = \sigma_1^2/\sigma_2^2$.

The manner of developing this fiducial test is as follows†: let

$$\left.\begin{aligned} &s_1 = \sqrt{\frac{\Sigma_1}{n_1(n_1-1)}}; \quad s_2 = \sqrt{\frac{\Sigma_2}{n_2(n_2-1)}}; \\ &d = (\bar{x}_1 - \bar{x}_2); \quad \delta = (\alpha_1 - \alpha_2); \quad \epsilon = (\delta - d); \\ &t_1 = \frac{(\bar{x}_1 - \alpha_1)}{s_1}; \quad t_2 = \frac{(\bar{x}_2 - \alpha_2)}{s_2}. \end{aligned}\right\} \qquad \text{.....(15)}$$

From (15) we obtain

$$\epsilon = (\delta - d) = s_2 t_2 - s_1 t_1. \qquad \text{.....(16)}$$

The fiducial distribution of δ is taken by Fisher to be the distribution obtained from (16) by treating d, s_1 and s_2 *formally* as fixed and allowing t_1 and t_2 to be distributed independently as t with degrees of freedom $(n_1 - 1)$ and $(n_2 - 1)$ respectively.

Now if A_1 and A_2 are any *constants* the distribution of $(A_2 t_2 - A_1 t_1)/\sqrt{A_1^2 + A_2^2}$ clearly depends only on A_1/A_2, n_1 and n_2. Hence we can theoretically determine

* See J. Neyman's paper, "Statistical Problems in Agricultural Experimentation". *J. Roy. Statist. Soc.* Supplement, II, No. 2 (1935), pp. 130–6. The sensitivity of t criteria to real population differences was seen to depend on f in a pronounced fashion only when f was very small (say < 6). In the present case the increase in sensitivity of u (with $f = 18$) over v (with $f = 6{\cdot}89$) will not be large.

† R. A. Fisher, *Ann. Eugen.* VI, Part IV (1935), p. 396.

a function $F(A_1/A_2, n_1, n_2, \gamma)$ such that the probability is γ that the inequality

$$|A_2 t_2 - A_1 t_1| > \sqrt{A_1^2 + A_2^2}\, F(A_1/A_2, n_1, n_2, \gamma) \qquad \text{.....(17)}$$

is satisfied. The corresponding statement in terms of fiducial probability is that the inequality

$$|\delta - d| > \sqrt{s_1^2 + s_2^2}\, F(s_1/s_2, n_1, n_2, \gamma) \qquad \text{.....(18)}$$

is satisfied with fiducial probability γ. The corresponding fiducial test of the hypothesis $\delta = 0$, which Fisher has suggested, consists in rejecting the hypothesis if

$$\left| \frac{d}{\sqrt{s_1^2 + s_2^2}} \right| > F(s_1/s_2, n_1, n_2, \gamma), \qquad \text{.....(19)}$$

where γ is now the level of significance. However, in repeated sampling from fixed populations π_1 and π_2 with $\alpha_1 = \alpha_2$, the repeated application of this test will not, in general, lead to rejection in the prescribed proportion, γ, of cases. Although the inequality (17), which can be written

$$\left| \frac{A_2(\bar{x}_2 - \alpha_2)}{s_2} - \frac{A_1(\bar{x}_1 - \alpha_1)}{s_1} \right| > \sqrt{A_1^2 + A_2^2}\, F(A_1/A_2, n_1, n_2, \gamma) \qquad \text{.....(20)}$$

is satisfied with probability γ whatever constant values are assigned to A_1 and A_2, this does not imply that the inequality (18) which can be written

$$\left| \frac{s_2(\bar{x}_2 - \alpha_2)}{s_2} - \frac{s_1(\bar{x}_1 - \alpha_1)}{s_1} \right| > \sqrt{s_1^2 + s_2^2}\, F(s_1/s_2, n_1, n_2, \gamma) \qquad \text{.....(21)}$$

will also be satisfied with the same probability γ.

It may be noted that in the case $n_1 = n_2 = 2$, Fisher has himself drawn attention to the difference between (20) and (21). As he has shown for that case*, the fiducial test involves the rejection of the hypothesis $\delta = \alpha_1 - \alpha_2 = 0$ at a 5% level of significance when a certain criterion T, which is a function of the sample observations only, numerically exceeds 12·7062. He shows also, however, that if we are sampling from fixed normal populations π_1 and π_2 for which $\delta = 0$, the probability of $|T|$ exceeding 12·7062 is only equal to 0·05 if $\theta = \sigma_1^2/\sigma_2^2 = 0$ or ∞; in general the probability will be less than 0·05.

That the statement (18) may be associated with a specially defined measure of fiducial probability which could be used by the experimenter as a guide in deciding whether to reject the hypothesis $\delta = 0$ is quite possible. But it seems to me important to make clear that the rule of the test involved in (19), if applied to repeated samples taken from fixed normal populations π_1 and π_2, would not lead in the long run on a proportion γ of occasions to the rejection of the hypothesis $\delta = 0$, when it is true, whatever be the value θ.

* *Ann. Eugen.* VII, Part IV (1937), p. 374.

8. *Exact tests.** Dr Bartlett has pointed out to me that the exact test which he gives for the case $n_1 = n_2 = 2$, is capable of easy generalization. For instance, if $n_1 = n_2 = n > 2$, let $l_{11}, l_{12}, \ldots l_{1,n-1}$ be $n-1$ linear functions of the observations in the first sample: let the l's be orthogonal to each other and to $\bar{x}_1$: further, let them all have expectation zero and standard deviation σ_1. Linear functions satisfying these conditions can always be defined. Similarly define $l_{21}, l_{22}, \ldots l_{2,n-1}$ for the second sample, these having standard deviation σ_2. Then $\sqrt{n}(\bar{x}_1 - \bar{x}_2)$ divided by $\sqrt{\left\{\sum_{i=1}^{n-1}(l_{1i}+l_{2i})^2\right\}/(n-1)}$ will be a criterion distributed as t with $(n-1)$ degrees of freedom, whatever the ratio σ_1^2/σ_2^2. Clearly a like test can be evolved when $n_1 \neq n_2$, the degrees of freedom of the corresponding t, then being one less than the smaller of n_1 and n_2. Bartlett would not advocate the use of this test in practice for the reason that it is not more efficient than using an inexact test based on v, and expressing the significance level of the sample as lying between two limits. The number of degrees of freedom for the criterion defined above is $n_1 - 1$ (if $n_1 < n_2$), whereas the effective number of degrees of freedom for v is never less than $(n_1 - 1)$ and may be as much as $(n_1 + n_2 - 2)$.

While on the subject of exact tests, it is of interest to note that other criteria of the form $(\bar{x}_1 - \bar{x}_2)/\sqrt{d\Sigma_1 + e\Sigma_2}$ may be less dependent on σ_1^2/σ_2^2 even than v. For instance, if n_1 and n_2 are both > 3 we might expect

$$z = (\bar{x}_1 - \bar{x}_2)\Big/\sqrt{\frac{\Sigma_1}{n_1(n_1-3)} + \frac{\Sigma_2}{n_2(n_2-3)}}$$

to be such a criterion. The reason for taking these particular values of d and e is that they give to σ_z^2 the same value both when $\theta = \sigma_1^2/\sigma_2^2 = 0$ and when $\theta = \infty$. The curve (c) in Fig. 2 shows the dependence of z on θ. Arranging the test so that the probability of rejection of the hypothesis $\alpha_1 = \alpha_2$ is 0·05 when $\theta = 1$, it is seen that no matter what θ, the probability of rejection departs from 0·05 less for z than for either of the criteria u and v. It is not proposed in the present paper to discuss whether tests such as these are of practical value.

9. *Summary.* Three tests of the hypothesis that the means of two normal populations are equal have been considered in some detail. The object has been to study how closely each of these controls the risk of rejecting the hypothesis when it is actually true. None of the tests was exact in the sense that it would control this risk precisely, whatever the unknown ratio θ of the variances of the two populations.

The first criterion u, which is the best criterion when it is known that θ is unity, can under certain circumstances be seriously biased when $\theta \neq 1$.

* By an exact test is meant one depending on a known probability distribution; that is, independent of irrelevant unknown parameters (e.g. in the present case independent of $\theta = \sigma_1^2/\sigma_2^2$). See, for instance, M. S. Bartlett, *Proc. Roy. Soc.* A, CLX (1937), p. 271.

The second criterion v, which employs separate estimates of the unknown variances of the two populations, was seen to be very much less liable to bias. Unless therefore it is definitely known that $\theta = 1$, the general use of v rather than u is worth serious consideration.

I agree with M. S. Bartlett's criticism of the third test, which has been put forward from considerations of fiducial probability. The bias of this test depends on θ, but I have not considered the relationship in any detail.

NOTE ON AN APPROXIMATION USED BY B. L. WELCH

BY ELIZABETH TANBURN, B.A.

In the preceding paper on the "Significance of the Difference between Two Means", B. L. Welch has considered two criteria, viz.

$$u = \frac{(\bar{x}_1 - \bar{x}_2)}{\sqrt{\frac{\Sigma_1 + \Sigma_2}{(n_1 + n_2 - 2)}\left(\frac{1}{n_1} + \frac{1}{n_2}\right)}}; \quad v = \frac{(\bar{x}_1 - \bar{x}_2)}{\sqrt{\frac{\Sigma_1}{n_1(n_1 - 1)} + \frac{\Sigma_2}{n_2(n_2 - 1)}}}.$$

He discusses the distribution of these in the case where the means α_1 and α_2 of the normal populations sampled are equal, but where the standard deviations σ_1 and σ_2 are not necessarily equal. He shows that u and v will be distributed approximately as ct_f, where c is a constant and t_f is distributed as "Student's" t having f degrees of freedom; c and f which are functions of n_1, n_2 and $\theta = \sigma_1^2/\sigma_2^2$ are given by equations (8) and (9) of p. 353 above.

The present writer has been studying the same problem both theoretically and also by means of practical sampling experiments and the results of this investigation will shortly be published. It may be of interest, however, to note here some points which have bearing on the approximation Welch has used. The approximation was made by fitting the quantities under the square roots in the criteria by Pearson Type III curves. The fitting was performed by making the Type III curves have the correct first two moments. This was a convenient method, but, of course, not the only one. We might for instance write $u \doteq ct_f$ and choose c and f so that the μ_2 and β_2 of ct_f are the same as the true μ_2 and β_2 of u, in other words represent u (and v) by a Pearson Type VII curve having the correct 2nd and 4th moments. In general, the objection to this is the more complicated form of the moments of u. (Also for very small samples these moments become infinite.) I have, however, obtained μ_2 and β_2 for u and v in one particular instance, viz. $n_1 = 5$, $n_2 = 15$, $\theta = \sigma_1^2/\sigma_2^2 = 0{\cdot}25$. They are given in the first line of Table I. Let us now compare them with the moments of Welch's approximation obtained by taking u (or v) $= ct_f$. For this we have

$$\mu_2 = \frac{c^2 f}{(f-2)}; \quad \beta_2 = \frac{3(f-2)}{(f-4)}.$$

TABLE I

	$\mu_2(u)$	$\beta_2(u)$	$\mu_2(v)$	$\beta_2(v)$
True moments	0·6002	3·493	1·1512	3·503
Moments of Welch's approximation	0·6011	3·509	1·1608	3·575

For u equation (8) gives $f = 15{\cdot}79$, $c^2 = 0{\cdot}5250$. For v equation (9) gives $f = 14{\cdot}44$, $c^2 = 1$. Substituting these we obtain the values given in the second line of Table I. The agreement with the true values is close enough to indicate that no great difference in the values of c and f would have occurred if Welch had used the moment method to represent u (or v) by a Type VII ("Student") curve rather than to represent the square of the denominator of u (or v) by a Type III (χ^2) curve.

A further comparison, which is worth making, is that between the approximate theoretical distribution of u and the actual distribution of 500 values of u which were obtained in a sampling experiment using Tippett's Random Numbers ($n_1 = 5$, $n_2 = 15$, $\theta = 0{\cdot}25$ as before). In the second line of Table II are shown the numbers of these sample u's whose absolute values lay between the limits given

TABLE II

$\lvert u \rvert$	0·000–0·362	0·362–0·500	0·500–0·627	0·627–0·777	0·777–0·969	0·969–1·266	1·266–1·538	1·538–1·874	>1·874
Frequency of samples	252	50	57	44	53	26	12	1	5
Approx. theoretical chances	0·50	0·10	0·10	0·10	0·10	0·05	0·03	0·01	0·01
Approx. expectations	250	50	50	50	50	25	15	5	5

in the first line. These limits are so chosen that, if the representation, $u = ct_f$, were exact (f being 15·79 and c^2 being 0·5250), then the true theoretical chances of u falling in the ranges would be those given in the third line of the table. The corresponding expectations for 500 samples are given in the last line. The sampling results are seen to agree very well with the theoretical distribution as it has been approximated.

COMPARISON BETWEEN BALANCED AND RANDOM ARRANGEMENTS OF FIELD PLOTS

By "STUDENT"

[With very deep regret the Editorial Committee has to report the death on 16 October 1937, of Mr W. S. Gosset, whose scientific contributions under the pseudonym of "Student" are well known to all statisticians. It is hoped to include some account of his life and work in the next issue of the *Journal*.

Mr Gosset had been working at the following paper during the past summer, and a fortnight before his death had discussed the draft, which is printed below, with Dr J. Neyman and Prof. E. S. Pearson. It was then agreed that certain points in sections 2 and 3 needed clarification and Mr Gosset proposed to undertake this work himself; unfortunately this final revision was never completed. Dr Neyman and Prof. Pearson have therefore added in a separate Note (pp. 380–88 below) some comments, for which they take full responsibility, regarding the points on which they know Mr Gosset had intended to enlarge.—Ed.]

In a paper read before the agricultural and industrial section of the Royal Statistical Society* I ventured to point out that the advantages of artificial randomization are usually offset by an increased error when compared with balanced arrangements. Prof. Fisher does not agree and has written a paper to test the difference of opinion that there is between us.†

In this paper I propose to set out as clearly as I can just what is this difference of opinion.

Next I propose to show that the conclusions of Prof. Fisher's paper all follow firstly from his having made use of a method of calculating the error of the "systematic" arrangements which I showed fourteen years ago would lead to just the misleading conclusions which he has found, and secondly to his not having compared like with like.

Thirdly, I will show that if he had not fallen into these pitfalls he would have been able to show that in the case which he took, a balanced arrangement does in fact give a slightly smaller error than his randomized one.

Fourthly, I will describe just what is to be expected when balanced arrangements are compared with random,‡ viz. that when the variance due to treatment is low compared with the error of the experiment, fewer significant results are obtained than with random arrangements, but when the variance due to treatment is high more significant results are obtained with balanced arrangements.

* "Co-operation in large-scale experiments," W. S. Gosset, Supplement to *J. roy. Statist. Soc.* III (1936), 115–22.

† "A test of the supposed precision of systematic arrangements," Barbacki and Fisher, *Ann. Eugen.* VII (1936), 189–93.

‡ Note that an arrangement can be both balanced and random and where this is practicable the aims of Prof. Fisher and myself are both satisfied.

Lastly, I will give in an appendix the results of some testing of balanced versus random arrangements on uniformity trials by Mr A. W. Hudson of Massey College, N.Z.

§ 1. The effect of lack of randomness on bias

It is almost invariably necessary, when applying mathematics to practical affairs, to replace the actual conditions by a set of simpler approximations with which the mathematics are capable of dealing, and mathematical statistics are no exception to this rule.

For example, the analysis of variance which is generally used to determine the error of agricultural experiments requires three assumptions to be made before we can apply the method strictly:

(1) The systems concerned are to have normal variation.

(2) The variances of like things should be equal.

(3) The sampling should be random.

(1) If, as is usual, the variation is not normal our argument will not be impaired unless the number of replications is very small, when departure from normality introduces an added uncertainty to the estimation both of mean and perhaps even more of variance.

(2) If, as often happens, the variances are not equal, as for example when we are pooling the variances of the yields of barleys which react differently to soils of different fertility, we shall not in general invalidate our conclusions appreciably, though in extreme cases attention should be paid to this source of error.

(3) If, however, the sampling be not random, there are such possibilities of drawing false conclusions that Prof. Fisher has introduced a system of artificial randomizing to ensure that the third condition is satisfied and brands all other systems invalid.

Nevertheless, it is possible, by balancing sources of error which would otherwise lead to bias, to obtain arrangements of greater precision which are nevertheless effectively random, by which I mean that the departure from randomness is only liable to affect our conclusions to the same sort of extent as do departures from normality or inequality of variances.

Lack of randomness can affect either the mean or the variance, and it is the first of these which is apt to lead to invalid conclusions. Thus Mr Yates has shown that it is practically impossible for anyone to select shoots of corn of average length by eye, and in fact none of the senses can be trusted to behave without bias. Those of taste or smell are peculiarly liable, and if comparisons are to be made it is necessary to avoid giving the least inkling of the order in which the samples are to be presented, in fact it is better to let it be known that it is a random order. In some cases the only way of avoiding bias is to withhold all knowledge of the object of the investigation from those taking part, though unfortunately this engenders a lack of interest in the proceedings.

Again, a promising experiment in nutrition was ruined by departure from randomness when the schoolmasters were allowed to adjust the supposed uneven effects of a chance selection of subjects for the Lanarkshire Milk Experiment, and in doing so managed to select, doubtless from the most humane motives, 10,000 children to receive milk who were significantly lighter and shorter than the 10,000 "controls" who did not.

In agricultural experiments there are obvious possibilities of bias affecting the mean in badly arranged experiments, for it is usual to find "fertility slopes" in most "uniformity" experiments, i.e. when an apparently uniform field is harvested in small-sized plots it is usual to find that the yield is higher in some parts than in others and tends to change more or less gradually from one place to another. Hence if plots of one variety are sited, whether systematically or by chance, nearer to one end of the experimental area than to the other, the mean is likely to be biased.

To take the simplest case of two varieties or treatments, the layouts

$$A\ B\ A\ B\ A\ B\ A\ B \text{ (systematic)}$$

and

$$A\ B\ A\ A\ B\ A\ B\ B \text{ (random)}$$

will both favour B if the field is more fertile on the right than on the left hand, the second rather more than the first.

On the other hand the layout

$$A\ B\ B\ A\ A\ B\ B\ A$$

is balanced with regard to a simple "linear" fertility slope, and the mean of neither A nor B will be biased except by departure from linearity.

It is, of course, possible to imagine particular variations in soil fertility which will bias the means of plots arranged in this manner, but with one exception they are of the same nature and lead to the same sort of bias—but usually to a smaller extent—as occurs with artificially randomized layouts.

The one exception is a periodic wave of fertility due to previous cultivations which happens to coincide in period with the width of an odd integral number of quartets, a not particularly likely occurrence.

Such layouts as $ABBA$ are termed balanced, and any number of treatments may be set in a balanced layout, as, for example, in the Latin square which is not only balanced but random as well, "thus conforming to all the principles of allowed witchcraft".

It is reasonable to expect that balanced layouts will on the whole be successful and that the mean will be less biased than in random, and this expectation is illustrated by some experimental sampling carried out by Mr A. W. Hudson of Massey College, N.Z., who tested balanced and random blocks against one another on three different uniformity trials. His results are given in the Appendix, and all

that need be said here is that in fifteen experiments the balanced layouts showed slightly more bias in three and less in twelve, the reduction of bias being very considerable in some of the twelve.*

And this brings me to a question which has often interested me. Suppose there are two treatments to be randomized—I take two for simplicity only—and suppose that by the luck of the draw they come to be arranged in a very *un*balanced manner, say *AAAABBBB*: is it seriously contended that the risk should be accepted of spoiling the experiment owing to the bias which will affect the mean if there is the usual fertility slope? For, as will be shown later, not only will the mean be biased, but the apparent precision will tend to be high, and misleading conclusions drawn much more often than the 1 or 5 % of the tables. It is of course perfectly true that *in the long run*, taking all possible arrangements, exactly as many misleading conclusions will be drawn as are allowed for in the tables, and anyone prepared to spend a blameless life in repeating an experiment would doubtless confirm this; nevertheless it would be pedantic to continue with an arrangement of plots known beforehand to be likely to lead to a misleading conclusion.

Let us suppose therefore—as indeed it is rumoured—that common sense prevails and chance is invoked a second time and that such an arrangement as *BBABBAAA* is offered; is this to be accepted? It is more likely to give a biased mean than *BABABABA*, but then of course it is random!

And if this is not to be used, how about *BBABABAA*? In short, there is a dilemma—either you must occasionally make experiments which you know beforehand are likely to give misleading results or you must give up the strict applicability of the tables; assuming the latter choice, why not avoid as many misleading results as possible by balancing the arrangements? And this, to do Prof. Fisher justice, is the direction towards which he is tending; in his paper with Dr Barbacki he treats for the first time of "randomized sandwiches" to which the objection is, not an appreciable increase of error, but the practical difficulty of working them.

To sum up, lack of randomness may be a source of serious blunders to careless or ignorant experimenters, but when, as is usual, there is a fertility slope, balanced arrangements tend to give mean values of higher precision compared with artificial arrangements.

Next, what is the effect of lack of randomness on the variance?

In a later section I will show that since in the "null" case, i.e. when no real treatment differences exist, the aggregate variance due to "treatments" and residual error is constant for all arrangements of treatments in the blocks, those with low actual error necessarily give high calculated values for the error and vice versa, the calculated error, however, varying much less than the actual in

* Mr Borden, of Hawaiian Sugar Planters' Association, Hawaii, has obtained similar results in similar experiments, and I have no doubt that this will always tend to happen.

ordinary experiments owing to the larger number of degrees of freedom of the residual error.

This, of course, has nothing to do with the origin of the experiment whether randomized or not.

If, however, the arrangement is "randomized" one can—*before the draw*—state accurately, subject to normality, etc., what the chance of getting any particular partition of variance between "treatment" and "residual error" will be in the "null" case. After the draw, when one particular arrangement has been chosen, it is often possible to be sure that the chance has changed in one direction or another without, however, being able to define exactly what it is.* In particular, balanced arrangements tend to have lower actual errors and higher calculated errors than would be expected by chance before a random selection is made, and this is so even if a degree of freedom is allocated to fertility slope, owing to the departure of the "slope" from linearity.

The consequence is that balanced arrangements more often fail to describe small departures from the "null" hypothesis as significant than do random, though they make up for this by ascribing significance more often when the differences are large.

Thus such departures from the "null" hypothesis as are found to be significant by balanced are likely to be larger than those found by randomized arrangements, and in particular those discovered in the "null" case itself—5 or 1 % as the case may be—tend to disappear altogether with balanced arrangements.

It will be seen then that the difference between Prof. Fisher and myself is not a matter of mathematics—heaven forbid—but of opinion. He holds that balanced arrangements may or may not lead to biased means according to the lie of the ground, but that in any case the value obtained for the error is so misleading that conclusions drawn are not valid, while I maintain that these arrangements tend to reduce the bias due to soil heterogeneity and that so far from the conclusions not being valid they are actually less likely to be erroneous than those drawn from artificially randomized arrangements. Further, that in the really important agricultural experiments which are carried out at more than one centre—and it was of these that I was speaking—the very slight disadvantage that an occasional result at an individual station may not be recognized as significant owing to over-estimation of the error at that station is more than offset by the greater precision of the experiment as a whole.

* This is analogous to the use of a life table to give the expectation of life. Thus the expectation of life of an Englishman of 40 can be referred to an appropriate table, but when we particularize the Englishman of 40 as a tin-miner or an agricultural labourer we know that the expectation is lower or higher than that given in the table without perhaps knowing very exactly by how much.

§ 2. Barbacki and Fisher

Such being our opinions, based in each case on *a priori* argument, Prof. Fisher rightly decided to put the matter to the test by assigning imaginary treatments to plots of which the yield had been determined in a uniformity experiment both on a random and on a balanced system, and published a paper,* of which he gives the following summary:

"1. This enquiry was carried out to test the truth of the opinion expressed by 'Student' that randomization achieves its object 'usually at the expense of increasing the variability when compared with balanced arrangements', and that one of the means available to experimenters of reducing the error is by adopting a 'regular balanced arrangement'.

"2. Using an extensive uniformity test it is found that the arrangements randomizing either pairs or sandwiches of half drill strips give smaller errors than the systematic arrangement advocated as more precise.

"3. As a consequence experimenters using the systematic arrangements systematically underestimate their errors.

"4. The error estimated from a systematic arrangement is ambiguous, and the experimenter has an arbitrary choice between several widely different estimates.

"5. Owing to the failure to furnish a valid estimate of error, 'Student's' test of significance is not approximately correct for systematic arrangements."

The particular arrangement which Prof. Fisher intended to test was the Half-Drill Strip† introduced by Dr Beaven some 14 years ago and widely used since then, but unfortunately half-drill strips are too large to lend themselves easily to testing on ordinary uniformity trials, and although Prof. Fisher has laid out eight pairs of half-drill strips on his uniformity trials he has not in fact compared them with a corresponding random arrangement but has cut them up transversely into 5-yard lengths and has compared the actual error of the large half drill strips with that calculated from the randomized‡ sheaf weights of which they are composed.

Now it happens that Dr Beaven had originally proposed to calculate the error of the half-drill strip from sheaf weights of this kind, and that I pointed out in this *Journal* 13 years ago§ that since such "sheaf weights" may be positively correlated such a method of calculating the error is fallacious.

This method of calculating the error has, of course, nothing to do with balanced arrangements, except that it was proposed by Dr Beaven, the author

* "A test of the supposed precision of systematic arrangements", Barbacki and Fisher, *Ann. Eugen.* VII, 189–93.

† Prof. Fisher prefers to call this the "Split Drill" Method, but though I agree that the name is more descriptive it is a pity to confuse the matter by a change of name after all these years. More particularly is it confusing to transfer the name "Half-Drill Strip" to small portions of the original half-drill strip as he has done, and I have called them by Dr Beaven's name of "Sheaf Weights".

‡ *Not very much randomized*; he compares corresponding pairs just as anyone else would.

§ "On testing varieties of cereals", *Biometrika*, XV (1923), 271–93.

of the half-drill strip; it might just as well be applied to random arrangements, as, for example, the "randomized pairs" of Prof. Fisher's experiment, each of which was actually harvested in six separate drills from which the error could have been equally erroneously calculated.

Prof. Fisher has therefore calculated the error of the half-drill strip by a method which I showed 13 years ago would be likely to give a fallaciously low value, and quite rightly has not used this method to calculate the error of his "randomized pairs": it is entirely due to this that he can draw conclusion (2) of his summary.

From this single fallacious conclusion he boldly generalizes to reach conclusion (3) which, as was shown by O. Tedin whom he quotes, is directly at variance with the facts. Conclusion (5) also follows solely from Prof. Fisher's faulty method and not from the balanced arrangement.

When the paper appeared I wrote a letter to *Nature* pointing this out, and that the actual error of the half-drill strip aggregate was in good conformity with that calculated from the weights of the whole strips.

In answering me Prof. Fisher replied that in that case the error of the "randomized sheaf weights" was so much smaller than that of half-drill strips that eleven times the area would have to be used to reduce the error of half-drill strips to that of "randomized sheaf weights" and further repeating his conclusion (4) with which I shall deal later.

Now one of the things that was noticed when uniformity trials first began was that the same piece of land laid out in large plots gave a very much larger error than if subdivided into small plots, and since half-drill strips were in this trial twelve times as large as "sheaf weights", Prof. Fisher's conclusion naturally follows since he is not comparing like with like.

Yet even so, those who have actually had to carry out agricultural experiments might very well prefer to work eleven times the area with ordinary agricultural methods and tools than have to sow and harvest 192 "randomized sheaf weights", if indeed that could be done at all under ordinary weather conditions.

Nevertheless, it is a fact that the error of this particular set of half-drill strips is unusually large. This arises partly because the number of repetitions is low but chiefly from the fact that the uniformity trial which Prof. Fisher chose to illustrate his argument showed a rather unusual feature due to faulty technique.

An examination of the original drills which were condensed to form the half-drill strips shows a periodicity, the averages of each eighth drill being for fifteen repetitions:

6739 7200 7839 6795 6689 7478 6897 6697

These variations are obviously not due to chance (for instance, the third drill gave the highest yield in twelve of the sets of eight and second highest in the other three) and are doubtless connected with some defect in the seed drill, probably the

tines were not evenly spaced, and this could possibly have been detected had it occurred to Mr Wiebe to examine the working of the drill before sowing.

The result is that since six of the eight drills were added up to form a "half-drill strip", then one drill omitted, and then another six, and so on, there was a periodic variation in fertility not coinciding in period with the width of the half-drill strip, and this, as I pointed out in the Appendix to my Royal Statistical Society paper, increases the calculated error but does not bias the mean.

For the same reason the correlation between the corresponding sheaf weights is very much higher than would usually be the case and full scope is thereby given to Prof. Fisher's faulty method of calculating the error.

Let us now deal with Prof. Fisher's fourth conclusion: "The error estimated from a systematic arrangement is ambiguous and the experimenter has an arbitrary choice between several widely different estimates."

We may observe in passing that this is another instance of Prof. Fisher's passion for generalizing on somewhat narrow foundations, for the possibility which he refers to is peculiar to the half-drill strip arrangement.

In the half-drill strip, however, it is possible either to calculate the error from such aggregates as $ABBA$ which I termed sandwiches in my paper to this *Journal* or from the separate parts of such aggregates, AB and BA, termed "pairs" by Prof. Fisher.

Of these the former is clearly the better if only there is a sufficient number of replications to give a good estimate of the error. As this is unusual it is generally best to give a degree of freedom to the fertility slope and calculate the error from "pairs".

Admittedly this tends to overestimate the error with the sort of results obtained in § 4. Faced with this choice, I personally choose the method which is most likely to be profitable when designing the experiment rather than use Prof. Fisher's system of *a posteriori* choice* which has always seemed to me to savour rather too much of "heads I win, tails you lose".

§ 3. A PROPERLY BALANCED ARRANGEMENT

It appears then that Prof. Fisher's paper is altogether irrelevant to the question at issue, but in order that Dr Barbacki's work may not be wholly wasted we can make a calculation of the error of a properly balanced arrangement of plots of the same size as the "randomized sandwiches" of which he has calculated the error.

For it will be noticed that Prof. Fisher's "systematic" arrangement, though "balanced" as "half-drill strips", is not so when regarded as a number of "sheaf weights": lateral balance is necessary.

The obvious layout is therefore to have the $ABBA$ arrangement in both directions.

* *Statistical Methods for Research Workers*, § 24.1 (5th ed.), p. 125.

Thus:

A	*B*	*B*	*A*	*A*	*B*	*B*	*A*	*A*	*B*		*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	
B	*A*	*A*	*B*	*B*	*A*	*A*	*B*	*B*	*A*		*B*	*B*	*B*	*B*	*B*	*B*	*B*	*B*	
B	*A*	*A*	*B*	*B*	*A*	*A*	*B*	*B*	*A*	etc. instead of:	*B*	*B*	*B*	*B*	*B*	*B*	*B*	*B*	etc.
A	*B*	*B*	*A*	*A*	*B*	*B*	*A*	*A*	*B*		*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	
A	*B*	*B*	*A*	*A*	*B*	*B*	*A*	*A*	*B*		*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	
B	*A*	*A*	*B*	*B*	*A*	*A*	*B*	*B*	*A*		*B*	*B*	*B*	*B*	*B*	*B*	*B*	*B*	
etc.											etc.								

This is merely a chessboard with fringes, each square being divided at harvest into four. The "squares" should be long and narrow, to gain the advantage of contiguity, and the comparisons should be made between adjacent long subplots of the different varieties. I have not seen this rather obvious arrangement mentioned before; it is admittedly no more suited for agricultural work than "randomized sandwiches", but it might be used in horticultural work, where the reduced "borders" would be of advantage, or for pot culture.

In this case we can start from Dr Fisher's Table II by reversing the signs of columns (ii), (iii), (vi), (vii), (x) and (xi) and calculate the error from an analysis of variance as follows:*

Variance due to	Degrees of freedom	Sum of squares of "split drill" differences
Longitudinal fertility slopes	12	887,171
Lateral fertility slopes	8	4,508,506
Varietal difference	1	2,741
Residual errors	75	3,988,681
Total	96	9,387,099

The difference between *A* and *B* is thus 513 g. and the S.D. of this difference 2259, as compared with 2353 calculated from "random sandwiches".

Thus, as we should expect, the difference is comfortably within the S.D., and the S.D. a little below that calculated from "randomized sandwiches", itself a partially balanced arrangement though random.

We see then that if a properly balanced arrangement is put down on the uniformity experiment of Dr Fisher's choice the error is found to be, as usual, less than his random arrangement, though not by much since "sandwiches" are themselves balanced.

* See note regarding this analysis on pp. 384–88 below. [ED.]

§ 4. The effect of "balancing" on the "validity" of conclusions

From *a priori* considerations—and Mr Hudson's and Mr Borden's experiments are in accordance with this expectation—it seems fairly certain (i) that "balancing" has no tendency to bias the mean, and (ii) that when there is a "fertility slope"—or anything corresponding to it, e.g. a time effect—the result will be to increase the apparent error but to decrease the real error. What effect has this on the "validity" of conclusions drawn from balanced experiments?

(i) *The case of blocks, randomized or balanced, judged by the z test*

Let us take the case of four treatments in six blocks giving fifteen degrees of freedom to the residual error and three for treatments, and let us suppose the arrangement put down on a uniformity trial.

Then, once the plots and blocks are marked out, the "total sum of squares" and the "sum of squares due to blocks" are fixed; the difference between these represents in all cases the eighteen degrees of freedom due to treatments and residual error, but will be divided between the two in different proportions according to the chosen arrangement of the treatments in the blocks. If the arrangement is random the frequency of any particular ratio is known to follow the z distribution, and owing to the skewness of this there will more often than not be a lower variance of the treatments with three degrees of freedom than of the residuals with fifteen.

If the arrangement is not random the frequencies will not follow the z distribution, e.g. with regular unbalanced arrangements the variance "due to treatment" will tend to be high compared with that of "residual error", while with regular balanced arrangements the reverse is the case. It will therefore be of interest to see what happens when a real "variance due to treatment" is imposed on uniformity trials which give ratios at different points of the z scale.

Thus it may be convenient to take as norm those uniformity trials which have the same variance for "means of treatments" as that calculated from the residuals and let this variance be σ^2. Then another set of trials may be considered of which the means have a variance of $0{\cdot}5\sigma^2$ and consequently a variance of "residual error" of $1{\cdot}1\sigma^2$, since $15 \times 1{\cdot}1 + 3 \times 0{\cdot}5 = 18$. This set may be taken to represent the tendency of balanced arrangements to produce low variance "due to treatment". A third set representing "unbalanced" arrangements may be taken with a means variance $1{\cdot}5\sigma^2$ and a variance calculated from residuals of $0{\cdot}9\sigma^2$.

All three of these occur, of course, in their proper proportions in random trials and are none of them uncommon. They are merely taken here as types.

In what follows I shall for convenience term the variance of means the *actual*

variance of error, σ_e^2, and the variance calculated from residuals the *calculated* variance of error.

Now suppose that a real variance due to treatment—measured without error, σ_T^2—be superposed upon the uniformity experiment. Then the calculated variance of error will be unaffected and the observed variance due to treatments will be $\sigma_T^2+\sigma_e^2+2r_{Te}\sigma_T\sigma_e$ and, since T and e are independent, the distribution of the observed variance can be calculated from the known distribution of r when there is no correlation, which in this case of four treatments is uniform between $+1$ and -1.

From this we can determine the probability that any given σ_T^2, superposed on any particular arrangement, will be deemed "significant" when compared with the corresponding "calculated variance of error".

The results of such calculations are given in the following table, which gives the probability of exceeding the 5 % limit of significance, or if preferred can be read as the percentages of "significant" results.

Value of σ_T^2/σ^2	Probability of obtaining significant result		
	Actual variance of error		
	$1{\cdot}5\sigma^2$	$1{\cdot}0\sigma^2$	$0{\cdot}5\sigma^2$
	Limit of significance		
	$2{\cdot}96\sigma^2$	$3{\cdot}29\sigma^2$	$3{\cdot}62\sigma^2$
0·5	0·22	0	0
1·0	0·41	0·18	0
1·5	0·51	0·34	0·03
2·0	0·58	0·45	0·22
2·5	0·63	0·53	0·36
3·0	0·68	0·60	0·48
3·5	0·72	0·66	0·57
4·0	0·76	0·71	0·66
4·5	0·79	0·76	0·73
5·0	0·82	0·80	0·80
5·5	0·85	0·84	0·86
6·0	0·88	0·88	0·92
6·5	0·90	0·91	0·97
7·0	0·93	0·94	1·00
7·5	0·95	0·98	—
8·0	0·97	1·00	—
8·5	0·99	—	—
9·0	1·00	—	—

This table illustrates the fact that arrangements which give an actual error less than the calculated fail to give as many "significant" results as those which

give larger actual errors up to a real treatment variance of about five times the average residual variance, at which point about 20 % of the experiments still fail to show significance in each case. When the real treatment variance rises above this point, the smaller the actual error the *more* are the significant results.

It is perhaps rather invidious to decide below what value of the real treatment variance "significant" results are misleading, but in any case it is clear that the fault of the arrangements with low actual variance is not lack of validity. On the contrary, conclusions drawn from experiments giving significant results by such arrangements are *more* valid in the ordinary sense of that word.

These arrangements have so far been considered as having arisen in a random manner, but by using balanced arrangements the proportion of arrangements having actual low errors is increased, and hence conclusions arrived at from balanced arrangements are more, not less, valid.

Nevertheless, it is clear that if it is required to calculate the error from an experiment carried out at a single station it is advisable not only to balance the experiment but to allow for the error eliminated by allocating a degree of freedom to the fertility slope. Even so it is likely that the actual error will be less than the calculated and the conclusions more valid than they appear to be.

(ii) *The case of half-drill strips judged by the t test*

I showed in the appendix to my paper on Co-operative Experiments that it is usually advantageous to allot one degree of freedom to the fertility slope, and that since fertility slopes are not usually strictly linear there is a tendency for the calculated error to be larger than the actual error. Let us illustrate this in the case of experiments carried out on the scale adopted by the N.I.A.B., namely, with ten pairs of comparisons; this is of course rather a small scale, and of the nine degrees of freedom one is allocated to the fertility slope and eight to the residual error of comparing the two varieties.

In this case we are to vary, not the position of treatments on a given piece of ground, but the pieces of ground on which a half-drill strip of ten pairs is set and the "norm" which we shall take is the case where, owing to a particularly uniform fertility slope, the calculated and the actual error exactly correspond with the standard error σ.

With this we can compare a case where the variance of actual error is $0{\cdot}5\sigma^2$ and the calculated error therefore $\left(1+\frac{0{\cdot}5}{8}\right)\sigma^2 = 1{\cdot}062\sigma^2$, i.e. standard errors $0{\cdot}71\sigma$ and $1{\cdot}03\sigma$. A tendency in this direction is, as noted above, common, since fertility slopes are naturally not uniform; on the other hand, when the fertility slope is small random sampling may give us a case where the actual error is larger than the calculated, let us say standard errors of $1{\cdot}22\sigma$ and $0{\cdot}97\sigma$.

Then in the three cases we find from the t table that the 5 % significance point is for the "norm" $2{\cdot}30\sigma$, for the low actual error $2{\cdot}37\sigma$, and for the high actual

error $2{\cdot}23\sigma$, while the actual errors are distributed normally with S.E.'s. σ, $0{\cdot}707\sigma$ and $1{\cdot}22\sigma$ and the percentage of "significant" results, i.e. those above the significant point calculated above, can be readily determined for values of the real (i.e. measured without error) differences between the two "varieties", say $A - B$.

These are given in the following table.

Variance of calculated error	$0{\cdot}94\sigma^2$	$1{\cdot}0\sigma^2$	$1{\cdot}06\sigma^2$
Variance of actual error	$1{\cdot}5\sigma^2$	$1{\cdot}0\sigma^2$	$0{\cdot}5\sigma^2$
S.E. calculated	$0{\cdot}97\sigma$	$1{\cdot}0\sigma$	$1{\cdot}03\sigma$
S.E. actual	$1{\cdot}22\sigma$	$1{\cdot}0\sigma$	$0{\cdot}707\sigma$
Limit of significance	$2{\cdot}23\sigma$	$2{\cdot}30\sigma$	$2{\cdot}37\sigma$
Value of $\frac{A-B}{\sigma}$	Probability of significant results		
0	0·07	0·02	0
0·5	0·01 0·08	0·04	0
1·0	0·16	0·10	0·03
1·5	0·27	0·21	0·11
2·0	0·42	0·38	0·30
2·5	0·59	0·58	0·58
3·0	0·74	0·76	0·81
3·5	0·85	0·88	0·95
4·0	0·93	0·96	0·99
4·5	0·97	0·99	1·00
5·0	0·99	1·00	—
5·5	1·00	—	—

It will be noticed that in the left-hand column there are two probabilities given opposite 0·5, 0·01 that a negative significant result and 0·08 that a positive significant result will be obtained. Fortunately such a case is almost impossible unless of course "randomized pairs" were used instead of a half-drill strip. What we are concerned with in practice is something which tends towards the right-hand column which, as in the case of the balanced blocks, errs by failing to give significant results when the difference to be measured is small, but from a value of about 2·55—at which all produce significant results in 60 % of trials—gives a higher percentage than when the calculated and actual errors are equal.

It is clear, therefore, that in this case too, conclusions drawn from a balanced arrangement are not less but more valid than if the arrangement had been random.

The above tables rather emphasize the well-known paradox that it is just when the experimenter is congratulating himself on the unusual smallness of his experimental error—unusual, that is, for the type of experiment and number of replications—that he is most likely to be betrayed into drawing false conclusions: for the small calculated error indicates a large actual error, and this whether the

arrangement be random or balanced, though it is likely to occur more frequently in the random.

In conclusion, I should like to emphasize the fact that when using the phrase criticized by Prof. Fisher I was concerned with co-operative experiments carried out at a number of different places.

Such experiments, as indeed all agricultural experiments, are only of value in so far as the venue is representative of the conditions under which the results of the experiment are to be applied, and so the result at any single station is not of any particular importance in itself but only in its interaction with the results obtained at the other stations, for only so can its representative nature be established.

To take a simple case a variety trial may indicate that one wheat will do better than another in heavy but not in light soils; such a conclusion is more likely to follow from an experiment carried out with a low real error and a correspondingly high calculated error at the individual stations than if a low calculated error gave "significant" results sporadically.

It is therefore important that the results should be determined with as little real error as possible, and the calculated error at each station is superseded by the error of the experiment as a whole.

APPENDIX GIVING MR A. W. HUDSON'S COMPARISONS OF RANDOM AND REGULAR ARRANGEMENTS IN UNIFORMITY TRIALS

Mr Hudson's account of his procedure is as follows:

"(i) Four, five or six imaginary treatments were allocated according to which was the most suitable to the full utilization of the data.

"(ii) These were allocated to blocks in a regular-balanced fashion and then to the same blocks randomwise, using various numbers of 'units' per individual plot.

"The regular arrangements were balanced by using two or four series in which the treatments in the second and fourth series were in opposite order to those in the first and third, thus:

1, 2, 3, 4, 1, 2, 3, 4, etc.
4, 3, 2, 1, 4, 3, 2, 1, etc.
2, 1, 4, 3, 2, 1, 4, 3, etc.
3, 4, 1, 2, 3, 4, 1, 2, etc.

or alternatively, where the shape of the individual plot permitted, only a single series, thus:

etc., 2, 1, 4, 3, 2, 1 Middle 1, 2, 3, 4, 1, 2, 3, etc."

TABLE I

Data from Journal of Agricultural Science, Vol. IV, *Part* 2, 1911.
Mercer and Hall. Mangold Plots

Number of rows ... 20
Units per row ... 10
} Total number of units 200, but only 160 used in first three.

B./Tr.	R. × U.	G.M.	Random			Balanced		
			Calculated S.E.	Dev. of T.M. from G.M.	Actual S.E.	Calculated S.E.	Dev. of T.M. from G.M.	Actual S.E.
20/4	1 × 2	656·4	6·63	− 3·3 − 5·7 + 1·6 + 7·5	5·84	6·73	+ 4·4 − 1·0 − 1·7 − 1·7	2·95
10/4	2 × 2	1312·8	14·16	+10·2 − 4·1 −12·2 + 6·3	10·15	14·42	− 0·8 + 8·3 − 1·9 − 5·4	5·54
10/4	1 × 4	1312·8	16·40	+12·7 −18·0 + 7·5 − 2·0	13·48	16·61	+16·3 − 5·8 − 6·8 − 3·5	10·92
8/5	1 × 5	1642·9	21·62	−32·8 −20·0 +27·8 + 4·3 +20·5	25·9	22·92	+15·0 −16·2 − 5·5 +19·7 −13·2	16·4
4/5	2 × 5	3285·7	50·78	+55·0 −25·0 − 4·2 +43·0 −68·7	50·6	54·62	+15·3 −15·7 −53·2 + 9·3 +44·5	36·7

Table headings: B./Tr. Blocks (replications) and treatments.
R. × U. Size of plot, rows × units.
G.M. General mean of all plots.
Calculated S.E., i.e. of means of treatments by analysis of variance.
Dev. of T.M. from G.M. Deviation of treatment means from general means.
Actual S.E., i.e. calculated from previous column.

TABLE II

Data from Journal of Agricultural Research, Vol. XLIV, *No.* 8, *April* 1932.

F. R. Immer. Yields of sugar beet.

Number of rows 60
Units per row 10
Total number of units 600

B./Tr.	R. × U.	G.M.	Random			Balanced		
			Calculated S.E.	Dev. of T.M. from G.M.	Actual S.E.	Calculated S.E.	Dev. of T.M. from G.M.	Actual S.E.
20/6	1 × 5	255·9	3·28	+ 1·1 + 3·3 + 0·6 + 0·4 − 6·2 + 0·8	3·22	3·27	− 1·1 − 3·7 − 2·1 + 6·1 + 1·1 0	3·40
10/6	2 × 5	511·9	8·42	−11·0 +13·0 − 1·6 − 7·2 +13·0 − 6·2	10·5	8·52	+ 7·5 + 5·1 − 5·8 −17·3 + 2·6 + 7·8	9·8
10/6	1 × 10	511·9	8·04	− 6·1 + 7·4 − 4·4 + 9·5 − 0·4 − 6·0	6·90	8·11	− 6·2 − 1·7 +10·4 − 2·7 − 3·7 + 3·7	6·08
4/6	5 × 5	1279·7	23·68	+68·2 +40·5 −41·6 − 6·3 −73·3 +12·5	52·1*	37·87	+11·4 + 5·4 −17·0 + 2·1 − 5·4 + 3·6	10·0

* This is a "significant" result—beyond the 1 % level—and it is perhaps a little unfortunate that it should have occurred in a mere sample of 21. It has, however, been checked both by Mr Hudson and myself.

TABLE III

Data from Journal of Agricultural Science, Vol. XXII, *Part* 2, *April* 1932.
Kalankar. Potatoes.

Number of rows ... 96 Units per row ... 6 Total number of units ... 576

B./Tr.	R. × U.	G.M.	Random A			Random B			Balanced		
			Calc. S.E.	Dev. of T.M. from G.M.	Actual S.E.	Calc. S.E.	Dev. of T.M. from G.M.	Actual S.E.	Calc. S.E.	Dev. of T.M. from G.M.	Actual S.E.
32/6	1 × 3	09·8	0·74	− 0·2 + 0·4 − 0·1 − 0·6 − 0·3 + 0·8	0·51	0·74	− 0·6 − 0·1 − 0·3 + 0·2 + 0·6 + 0·3	0·44	0·74	− 0·5 + 0·3 + 0·9 − 0·1 + 0·3 − 1·0	0·67
16/6	1 × 6	139·6	1·52	− 1·9 0 + 2·7 − 1·9 + 0·7 + 0·4	1·74	1·49	+ 1·1 + 1·8 − 3·1 + 1·2 + 1·1 − 2·1	2·05	1·55	− 1·4 + 1·3 + 0·9 + 0·7 0 − 1·5	1·20
16/6	2 × 3	139·6	2·16	+ 0·2 − 1·1 − 2·9 − 0·1 + 0·4 + 3·5	2·10	2·19	− 2·8 + 1·5 + 0·8 + 0·2 + 0·1 + 0·2	1·52	2·20	+ 0·2 − 0·3 − 2·1 − 0·5 + 2·0 + 0·8	1·38
8/6	2 × 6	279·2	5·35	+ 8·8 + 0·9 − 5·4 − 1·4 − 2·8 − 0·1	4·84	5·47	+ 0·8 + 6·0 + 2·1 − 3·8 − 1·1 − 4·1	3·83	5·56	+ 3·0 + 3·3 − 2·0 − 3·3 0 − 1·0	2·68
8/6	4 × 3	279·2	5·67	+ 1·3 + 6·1 + 1·1 − 3·9 − 3·5 − 1·1	3·71	5·58	+ 8·0 − 1·3 − 3·8 − 0·1 − 4·4 + 1·6	4·52	5·60	+ 2·4 − 4·4 − 2·2 − 2·7 − 0·9 + 7·7	4·42
4/6	8 × 3	558·4	33·3	−12·1 − 7·2 +41·0 +35·8 −19·0 −38·7	31·7	35·85	− 5·9 +39·0 +10·8 −16·9 −12·5 −14·6	21·6	37·76	− 3·3 −10·8 + 5·6 − 0·6 + 1·1 + 7·8	6·7

The above experimental work must not be taken as an attempt at a proof that balanced arrangements are likely to give a lower error than random unbalanced arrangements; that seems to me obvious, and it is for those who wish to disprove the obvious to obtain evidence in support of their eccentric opinions, but it does give an interesting illustration of what is likely to happen in practice, and I print it in the hope that it will help to clarify other people's ideas as it has mine.

NOTE ON SOME POINTS IN "STUDENT'S" PAPER ON "COMPARISON BETWEEN BALANCED AND RANDOM ARRANGEMENTS OF FIELD PLOTS"

By J. NEYMAN and E. S. PEARSON

During the summer of 1937 "Student" discussed the subject of this paper with one of us on several occasions. The paper was some months in preparation "not so much", he wrote, "owing to lack of time as to lack of inclination to controversy". He was particularly anxious, however, that it should contain as clear a statement as possible of his views on balanced and random arrangements, and when in the middle of September he sent us a final draft for comment, he asked us to make any suggestions for improvement we could think of. We told him in the first place that we felt that the reader who was not very familiar with the literature might find a little difficulty in following the points at issue between himself and Prof. Fisher. Secondly, on a smaller point, we suggested that he should explain somewhat more fully the analysis of variance carried out in his section 3.

These suggestions he welcomed, and a letter written four days before his death indicated that he was in the middle of adding to the paper some comments on these points. What form these additions would have taken we cannot tell with certainty, but we feel that it is right for us to add in a separate note a little fuller explanation of some of the points raised in sections 2 and 3 of the paper.

1. *The half-drill strip method.* In his paper "On Testing Varieties of Cereals" (*Biometrika*, vol. xv, 1923, p. 285 *et seq.*), "Student" described the half-drill strip method somewhat in the following terms:

When sowing, the seed box of the drill is divided into two across the middle, and the middle coulter put out of action. The seed of the two varieties, say A and B, is put in the seed box, one on each side of the division. Thus when sowing a drill strip, one half (i.e. six of seven rows) is sown with the variety A and the other half with the variety B. On turning the drill at the end, the next strip is sown so that two half-strips of the same variety B are next each other, but care is taken to leave an interval between the two drill strips exactly equal to the gap in the middle of each drill strip between the two varieties. It requires careful steering but it can be done.

When the experimental field is sown, we get first a single half-drill strip of variety A, then two of the other variety B, then two of A and so forth, ending with a half-drill strip of A. This ending is necessary in order to discount any fertility slope from one end to the other of the field. The situation is illustrated in Fig. 1.

Four consecutive half-drill strips form a sandwich. The idea of this arrangement is based on the empirical fact that the changes in soil fertility that are met with in uniform fields which might be chosen for trials are frequently "monotonic", that is the fertility increases gradually, though perhaps not always uniformly, from one end of the field to the other. If this is so, then the variety B may be favoured by its position in the first pair of half-drill strips, but then it will have a disadvantage, of about the same importance, in the second, and so on. Consequently sandwiches, considered as units of the experiment, will be well balanced and are likely to provide equal conditions of comparison of the two varieties.

"Student" considered three different ways of treating statistically the results of half-drill strip experiments.

(*a*) *Method of pairs.* This consists in considering the difference $A_i - B_i$ between the yields of A and B in each of the pairs of half-drill strips as independent observations. Consequently the variance of the mean, $\overline{A-B}$, of such differences would be estimated by, say,

$$s_1^2 = \frac{\Sigma (A_i - B_i - \overline{A-B})^2}{2n\,(2n-1)}, \qquad \ldots\ldots(1)$$

where $2n$ denotes the number of pairs of half-drill strips, while n is the number of sandwiches.

(*b*) *Method of sandwiches.* Here the differences, say,

$$\left.\begin{aligned} A_1 - B_1 - B_2 + A_2 &= \Delta_1 \\ A_3 - B_3 - B_4 + A_4 &= \Delta_2 \end{aligned}\right\}, \qquad \text{.....(2)}$$

are considered as single independent observations, and the variance of their mean $\overline{\Delta}$ is estimated by

$$s_2^2 = \frac{\Sigma(\Delta_i - \overline{\Delta})^2}{n(n-1)}. \qquad \text{.....(3)}$$

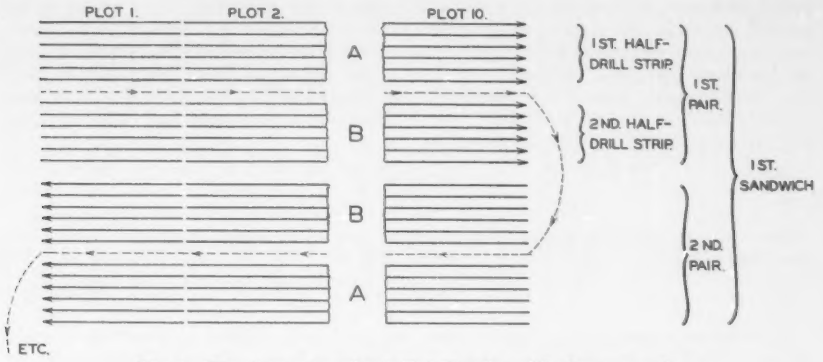

Fig. 1. Directions of sowing half-drill strips with six rows each.

(*c*) Finally, "Student" considered the possibility of artificially multiplying the number of observations by subdividing half-drill strips transversely into several portions. Fig. 1 shows the subdivision of the half-drill strips forming the first sandwich, each into $m = 10$ such portions, which "Student" calls plots. If a_{ij} and b_{ij} denote the yields of the varieties A and B from the jth plots of the ith pair of the half-drill strips, while $\overline{a-b}$ is their arithmetic mean, then its variance could be estimated by

$$s_3^2 = \frac{\Sigma\Sigma\{a_{ij} - b_{ij} - (\overline{a-b})\}^2}{2nm(2nm-1)}. \qquad \text{.....(4)}$$

The values such as a_{ij} and b_{ij} are called sheaf weights.

In discussing these three methods, "Student" was aware of the fact that the three formulae for s_1, s_2, and s_3 are not exact, because of the correlations existing between adjacent half-drill strips and between plots adjacent within each of the half-drill strips. Having in view the possibility of linear or nearly linear variation of soil fertility, "Student" indicated that s_1 will tend to overestimate the actual inaccuracy of the experiment. Therefore, he really advised the use of the method of sandwiches. As to the method (*c*), "Student" discussed it at length, concluding that it was likely to be misleading. In fact, the total of the differences $a_{ij} - b_{ij}$ is exactly equal to that of $A_i - B_i$. On the other hand, the number of the former is m times that of the latter. Finally, the cultivation processes and other circumstances result in the fact that the values of $a_{ij} - b_{ij}$, as calculated for the same pair of half-drill strips, tend to be alike. The result is that the greater the number of "plots", the smaller the value of s_3, and this decrease is purely artificial, connected with the method of calculating and not with the accuracy of the experiment. "Student" illustrates these facts on Beaven's data and adds the following footnote which we think is significant:

"A fallacy arising from a similar neglect of correlation has come under my notice in some American work, but there the absurdity is more easily demonstrated. In the *Journal of the American Society of Agronomists*, vol. IX, 1917, p. 138, A. G. McCall proposed that in order to save the trouble of harvesting and weighing 1/10th acre plots, a number of square yards

should be cut out and harvested separately, the square yards being taken systematically throughout the 1/10th acre plot, and the yield per acre calculated from these square yards. So far, so good, by taking enough square yards the slight loss of accuracy may perhaps be made up by gain in time or feasibility of operating. But in 1919, Arny and Steinmetz, *Journal of the American Society of Agronomists*, vol. XI, pp. 88, 89, applying this method, compared the error of the yield calculated from a few square yards cut from each of a number of 1/10th acre plots with that calculated from the 1/10th acre plots themselves. They found it substantially greater, but, say they, by increasing the number of square yards cut from each 1/10th acre plot to n, we can decrease the error in the proportion $1/\sqrt{n}$, and so we can actually determine the yield more accurately by weighing up 10 or 20 square yards than by weighing up the whole half acre. It is rather surprising that they did not realise that there are 484 square yards in 1/10th acre, so that by taking 484 square yards they would be likely to be more accurate than if they took any lesser number and a fortiori tremendously more accurate than they would be if they took the same 484 square yards and called it 1/10th acre! Of course their formula also should be

$$\sigma\sqrt{\frac{1+r(n-1)}{n}}, \qquad \text{.....(5)}$$

where r is the correlation between the yields on the square yards composing 1/10th acre plots, and not

$$\sigma\sqrt{\frac{1}{n}}. \qquad \text{.....(6)}$$

"The same fallacy has been used to extol the 'rod row' method of determining yield, i.e., the method of cutting along the drill a row one rod in length to represent the yield of the plot from which it is cut."

The correlation between sheaf weights (i.e. yields of "plots") within one half-drill strip is clearly seen in Fig. 2, which represents, in the form used by Barbacki and Fisher, the experimental results of Gustav A. Wiebe referred to by "Student" in the present paper.

Wiebe sowed 125 rows of wheat, divided them transversely into twelve portions of 5 yards each, and harvested and weighed separately the yields of the 1500 portions arranged in twelve columns. In order to obtain fuller information on certain points, we wrote to him and he has kindly supplied us with the following details for which we are very grateful: (1) The direction of ploughing was that of the rows, from west to east. (2) The direction followed by the drill, which sowed eight rows at a time, was also *always* from west to east, i.e. it did not proceed backwards and forwards in the usual manner suggested in Fig. 1, but returned outside the field each time to start again from the west. (3) As "Student", after examining the original data, has suggested, there was a fault in the drill not realized until after the experiment was completed, in particular holes Nos. 3 and 6 having a higher grain delivery than the others.

Dealing separately with each column, Barbacki and Fisher totalled the yields of single rows in groups of six, omitting one row between each consecutive pair of groups. The four-figure numbers in the above diagram represent the results they have obtained and used for further calculations. Each number corresponds exactly to what "Student" called a sheaf weight. To test the relative accuracy of systematic half-drill strips, the authors assigned to particular rows of plots imaginary treatments, as shown in Fig. 2. Then they proceeded to calculate (1) the actual difference between the mean of yields of A and B and (2) the estimate of the S.D. according to formula (4), that is to say they followed exactly the method which "Student" was at pains to advise not to follow, as it leads to an underestimation of the S.D. The results obtained by Barbacki and Fisher confirm what "Student" expected: the estimate of error, as calculated from sheaf weights, is too low. It is also easy to see that "Student" was right in giving the reason for the underestimation of the error variance, if sheaf weights are used for this purpose. He pointed out that the plots within the same half-drill strip, i.e. in the same row in Fig. 2, tend to be correlated. They actually are highly correlated in the experimental data used by Barbacki and Fisher. To illustrate this

North

A 4410 **126**	A 4035 **115**	A 3865 **110**	A 3640 **104**	A 3650 **104**	A 3985 **114**	A 3490 **100**	A 3330 **95**	A 3358 **96**	A 3712 **106**	A 3487 **99**	A 3781 **108**
B 3950 **113**	B 3865 **110**	B 3295 **94**	B 2960 **84**	B 2925 **83**	B 3685 **105**	B 3400 **97**	B 3040 **87**	B 2889 **82**	B 3195 **91**	B 3496 **100**	B 3576 **102**
B 4185 **119**	B 4075 **116**	B 3325 **95**	B 2860 **82**	B 2965 **85**	B 3770 **108**	B 3240 **92**	B 2735 **78**	B 2764 **79**	B 3460 **99**	B 3273 **93**	B 3442 **98**
A 3785 **108**	A 3515 **100**	A 3255 **93**	A 2815 **80**	A 2630 **75**	A 3295 **94**	A 2875 **82**	A 2630 **75**	A 2775 **79**	A 3040 **87**	A 2940 **84**	A 3152 **90**
A 3870 **110**	A 3780 **108**	A 3660 **104**	A 2980 **85**	A 2650 **76**	A 3250 **93**	A 2925 **83**	A 2915 **83**	A 2933 **84**	A 3277 **93**	A 3042 **87**	A 3363 **96**
B 3910 **112**	B 3690 **105**	B 3705 **106**	B 3050 **87**	B 2910 **83**	B 3630 **104**	B 2985 **85**	B 3130 **89**	B 2986 **85**	B 3040 **87**	B 2778 **79**	B 3123 **89**
B 3890 **111**	B 3695 **105**	B 3720 **106**	B 2990 **85**	B 2970 **85**	B 3315 **95**	B 2910 **83**	B 2985 **85**	B 2851 **81**	B 2635 **75**	B 2906 **83**	B 3081 **88**
A 4190 **120**	A 3970 **113**	A 4335 **124**	A 3350 **96**	A 3325 **95**	A 3870 **110**	A 3120 **89**	A 3015 **86**	A 3097 **88**	A 2909 **83**	A 2936 **84**	A 3628 **103**
A 4170 **119**	A 4070 **116**	A 4455 **127**	A 3610 **103**	A 3365 **96**	A 3460 **99**	A 2970 **85**	A 2855 **81**	A 2877 **82**	A 2834 **81**	A 3020 **86**	A 3632 **104**
B 4015 **115**	B 4480 **128**	B 4730 **135**	B 3805 **109**	B 3375 **96**	B 3545 **101**	B 3080 **88**	B 2810 **80**	B 2794 **80**	B 2974 **85**	B 2770 **79**	B 3805 **109**
B 4150 **118**	B 4755 **136**	B 5065 **144**	B 4125 **118**	B 3550 **101**	B 3740 **107**	B 3425 **98**	B 2690 **77**	B 2789 **80**	B 2810 **80**	B 2895 **83**	B 3695 **105**
A 4190 **120**	A 4740 **135**	A 5265 **150**	A 4415 **126**	A 3675 **105**	A 3965 **113**	A 3685 **105**	A 3030 **86**	A 2782 **79**	A 2904 **83**	A 3080 **88**	A 3798* **108**
A 4095 **117**	A 5075 **145**	A 5495 **157**	A 4270 **122**	A 3760 **107**	A 4010 **114**	A 3695 **105**	A 3255 **93**	A 2759 **79**	A 3118 **89**	A 3287 **94**	A 3547 **101**
B 3805 **109**	B 4360 **124**	B 4415 **126**	B 3870 **110**	B 3585 **102**	B 3785 **108**	B 4025 **115**	B 3300 **94**	B 3199 **91**	B 3407 **97**	B 3473 **99**	B 3572 **102**
B 4005 **114**	B 4225 **120**	B 3840 **110**	B 3800 **108**	B 3780 **108**	B 3780 **108**	B 4025 **115**	B 3710 **106**	B 3564 **102**	B 3616 **103**	B 3539 **101**	B 3853 **110**
A 3700 **106**	A 4325 **123**	A 3550 **101**	A 3455 **99**	A 3540 **101**	A 3660 **104**	A 3980 **114**	A 3705 **106**	A 3577 **102**	A 3759 **107**	A 3558 **101**	A 3673 **105**

West

East

South

* Probably through a printer's error, this number was given as 2798 in Barbacki and Fisher's Table I; the modified number leads to the total yield which they give in the text.

Fig. 2. Yields of plots on Wiebe's field as arranged by Barbacki and Fisher. N.B. The main figures are yields; the figures below in heavier type express each yield as a percentage of the mean.

point, their original figures were calculated as percentages of the average sheaf weight and the results are given in heavier type in the figure. It is seen that, with a single exception, the yields of all plots in the first row exceed those of corresponding plots in the second and that a similar tendency appears in the other rows. Roughly speaking, each column of plots tells about the same story as any other column and as would the sums of all twelve plots along the rows. This is just the circumstance "Student" had in mind when stating that the use of sheaf weights for the estimation of error variance must lead to its underestimation.

Barbacki and Fisher tried to show that the systematically analysed half-drill strips vitiate "Student's" t-test. For this purpose they split their data into six hypothetical experiments. The first of these was supposed to include the first and the seventh columns of plots, the second experiment, the two next columns, second and eighth, etc. They have obtained exactly what could be expected from "Student's" warning concerning the correlation between the sheaf weights within one half-drill strip: each of their hypothetical experiments was an approximate replicate of any other among them, in particular all the six t's were of the same sign.

The application of statistical analysis to Wiebe's data has emphasised two points of importance. In the first place before carrying out a half-drill strip experiment it is very necessary to examine the delivery of the separate holes in the drill. Secondly in using available uniformity trial data to investigate the relative efficiency of various experimental lay-outs, care must be taken that the plots selected are so chosen that possible faults in the drill (often hard to detect after the event) do not add spurious fluctuations in yield to those genuinely due to change in the level of fertility. Had the sheaf weights chosen by Barbacki and Fisher been seven drills broad instead of six, with one intervening row omitted, the correlation effect probably would have been reduced.

It should be mentioned that in a later paper* "Student" has advanced a method of dealing with pairs of half-drill strips, allowing for a linear fertility gradient, which has a definite justification on the grounds of the theory of probability. In fact it is a direct consequence of the following system of hypotheses:

(*a*) Within each pair of half-drill strips, the fertility of soil varies linearly and the slope is the same for all pairs.

(*b*) The treatments compared react similarly to changes in soil fertility; if a strip, P_1, is better than another, P_2, for one treatment, then it is also to about the same extent better for the other treatment.

(*c*) Technical errors of experimentation are independent of changes in soil fertility and of the treatments, and are normally distributed about zero.

It is, of course, uncertain whether in any particular case the changes in the fertility level over the field can be represented with sufficient accuracy by portions of straight lines with constant slope. But this unescapable difficulty is of a kind which arises always whenever we try to deal mathematically with any objects of the outside world. Mathematics deals with mathematical conceptions, not with real things and we can expect no more than a certain amount of correspondence between the two. This applies equally to mathematical treatment of other experimental designs such as randomized blocks and Latin squares in spite of the random assignment of treatments, though strong opinions have sometimes been expressed to the contrary. Whether in the case of the half-drill strips, this correspondence is, for practical purposes, satisfactory or not must be tested empirically. This kind of test, having in view the question as to whether the above hypotheses (*a*), (*b*) and (*c*) do usually lead to a satisfactory approximation of the actual level of fertility on experimental fields, is now being carried out and it is hoped that the results will soon be published.

2. *The balanced arrangement of "Student's" section* 3. In this section "Student" has suggested a form of completely balanced arrangement which might be applied to the 192

* This method seems first to have been described and illustrated by "Student" in his article on "Yield Trials" published in *Baillière's Encylopedia of Scientific Agriculture*, II, London (1931), 1358–60. In the algebraic table given later in his paper in the Supplement to *J. roy. Statist. Soc.* III (1936), 121, it seems necessary to read $S(A-B)^2 - n(\overline{A-B})^2$ for $S(A-B)^2$.

plots into which Fisher and Barbacki have chosen to divide Wiebe's field. His main purpose here was to show that for such an arrangement the total treatment difference (513 g.) is comfortably within the standard error (2259). The much larger total treatment difference (5875 g.) given by Fisher and Barbacki (*loc. cit.* p. 191) applies to an arrangement which was not balanced horizontally, and is therefore irrelevant to any fair comparison of balanced versus random arrangements of sheaf weights. In examining the method which "Student" followed in analysing his results, we have not however been able to reproduce exactly the analysis of variance table which he gives on p. 371 above. While the difference between the estimate of error, which he gives (2259) and that which we believe is correct on the hypothesis from which he appears to start (2385), is not in any sense vital to his argument, we think it may be worth while examining the hypothesis regarding the fertility level in the experimental field which seems to underly the statistical treatment of the data that he has outlined.

Consider then the arrangement of $16st$ plots in $4s$ rows and $4t$ columns and assume that two varieties only, A and B, are sown on these plots according to the scheme shown below. For the data of Fig. 2 $s=4$, $t=3$.

	1	2	3	4	...	$4t-1$	$4t$
1	A	B	B	A		B	A
2	B	A	A	B		A	B
3	B	A	A	B		A	B
4	A	B	B	A		B	A
5	A	B	B	A		B	A
⋮							
$4s-1$	B	A	A	B		A	B
$4s$	A	B	B	A		B	A

It will be assumed that the plots are oblong and narrow, their longer side being parallel to the rows of the diagram. Each pair of the plots, with their longer sides adjacent, will provide a comparison between A and B. There will be $2s$ such pairs of plots within each of $4t$ columns, or $8st$ pairs in all. It is clear, however, that any such comparison within a pair of plots by itself would not be a fair one, as the two plots forming the pair are not likely to be of the same fertility. It will be explicitly assumed that their fertility is different and we shall denote by F_{ij} for $i=1, 2, \ldots, 2s$, $j=1, 2, \ldots, 4t$, the advantage of the lower plot over the upper within any of $8st$ pairs into which the whole field may be divided. We shall further denote by $F_{..}$, the average of all the numbers F_{ij}, by $F_{i.}$ their average within the ith row and by $F_{.j}$ the average within the jth column. Consequently, if we take into consideration the ith pair of plots within the jth column (pair (ij) for short) and denote by A_{ij} the average of the true yields which the variety A is able to give on these plots, then its true yield on the upper plot would be

$$A_{ij}-\tfrac{1}{2}F_{ij} \qquad \ldots\ldots(7)$$

and that on the lower one

$$A_{ij}+\tfrac{1}{2}F_{ij}. \qquad \ldots\ldots(8)$$

This could be written for any variety and for any field; the above formulae are meant to explain the notation, but do not imply any hypothesis concerning the experimental field or varieties. We shall now formulate the hypotheses leading to the new method suggested by "Student", which we assume he must have had in mind when writing his paper.

(1) We shall assume that the advantage of the lower over the upper plot in a pair is of the same magnitude for both varieties, A and B. Consequently, the true yields of the variety B on the plots of the (ij) pair will be

$$A_{ij}-\tfrac{1}{2}F_{ij}-\Delta \quad \text{and} \quad A_{ij}+\tfrac{1}{2}F_{ij}-\Delta, \qquad \ldots\ldots(9)$$

respectively, where Δ denotes the difference between the yields $A-B$ if sown in identical conditions.

(2) We shall make a certain hypothesis regarding the variability of soil over the field or,

what comes to the same thing, regarding the values of the F_{ij}. Notice first that whatever the field may be, we can write

$$F_{ij} = F.. + (F_{i.} - F..) + (F._j - F..) + (F_{ij} - F_{i.} - F._j + F..)$$
$$= F.. + R_i + C_j + \eta_{ij}, \qquad \text{.....(10)}$$

where R_i means the correction to be added to $F..$ to obtain the average of the F_{ij} within the ith row of pairs and C_j a similar correction for columns. We shall assume that the selection of the field for the experiment was a careful one so that the variation of fertility over it is regular, in the sense that the first three terms in the right-hand side of (10) represent sufficiently accurately the left-hand side, thus

$$F_{ij} = F.. + R_i + C_j \quad \text{for } i = 1, 2, \ldots, 2s;\ j = 1, 2, \ldots, 4t. \qquad \text{.....(11)}$$

It will be noticed that this implies

$$\Sigma(R_i) = \Sigma(C_j) = 0. \qquad \text{.....(12)}$$

Substituting (11) into (7), (8) and (9), we shall obtain the hypothetical set-up of "true yields" of the varieties A and B, which they are able to give on both plots of the (ij) pair

$$\left.\begin{array}{ll} A_{ij} - \tfrac{1}{2}(F.. + R_i + C_j), & A_{ij} - \tfrac{1}{2}(F.. + R_i + C_j) - \Delta, \\ A_{ij} + \tfrac{1}{2}(F.. + R_i + C_j), & A_{ij} + \tfrac{1}{2}(F.. + R_i + C_j) - \Delta. \end{array}\right\} \qquad \text{.....(13)}$$

The following table represents the set-up within the pairs of the first few rows and columns.

TABLE I

Expected yield of A and B

	1st column of pairs	2nd column of pairs	3rd column of pairs	4th column of pairs
1st row of pairs	$A_{11} - \frac{1}{2}(F.. + R_1 + C_1)$ $A_{11} + \frac{1}{2}(F.. + R_1 + C_1) - \Delta$	$A_{12} - \frac{1}{2}(F.. + R_1 + C_2) - \Delta$ $A_{12} + \frac{1}{2}(F.. + R_1 + C_2)$	$A_{13} - \frac{1}{2}(F.. + R_1 + C_3) - \Delta$ $A_{13} + \frac{1}{2}(F.. + R_1 + C_3)$	$A_{14} - \frac{1}{2}(F.. + R_1 + C_4)$ $A_{14} + \frac{1}{2}(F.. + R_1 + C_4) - \Delta$
2nd row of pairs	$A_{21} - \frac{1}{2}(F.. + R_2 + C_1) - \Delta$ $A_{21} + \frac{1}{2}(F.. + R_2 + C_1)$	$A_{22} - \frac{1}{2}(F.. + R_2 + C_2)$ $A_{22} + \frac{1}{2}(F.. + R_2 + C_2) - \Delta$	$A_{23} - \frac{1}{2}(F.. + R_2 + C_3)$ $A_{23} + \frac{1}{2}(F.. + R_2 + C_3) - \Delta$	$A_{24} - \frac{1}{2}(F.. + R_2 + C_4) - \Delta$ $A_{24} + \frac{1}{2}(F.. + R_2 + C_4)$
3rd row of pairs	$A_{31} - \frac{1}{2}(F.. + R_3 + C_1)$ $A_{31} + \frac{1}{2}(F.. + R_3 + C_1) - \Delta$	$A_{32} - \frac{1}{2}(F.. + R_3 + C_2) - \Delta$ $A_{32} + \frac{1}{2}(F.. + R_3 + C_2)$	$A_{33} - \frac{1}{2}(F.. + R_3 + C_3) - \Delta$ $A_{33} + \frac{1}{2}(F.. + R_3 + C_3)$	etc.
	etc.	etc.		

(3) The third hypothesis concerns the difference, ϵ, between these "true yields" and those which might be observed. It will be assumed that the formulae (13) make a full allowance for both differences between the varieties (Δ) and the soil variation (components A_{ij}, $F..$, R's and C's) and that therefore the difference ϵ is due only to inevitable random technical errors of experimentation, normally distributed about zero with an unknown variance.

Are these hypotheses satisfied in practice? It is difficult to say for certain but it seems likely that they might be. In fact, the number of arbitrary constants is very considerable and the range of variety in fertility levels which could be constructed by varying them, even within narrow limits, is enormous. Nor has the scheme any sort of rigidity involved in the original "Student's" set up for the half-drill strips, where the fertility slope was assumed to be constant throughout the field. Now the slope can change from pair to pair within each column and row, being sometimes negative and sometimes positive. All these considerations

suggest that if the choice of the experimental field is not very unlucky, then the changes of its fertility can be approximated by the above scheme with a great degree of accuracy.

Granting this, we may proceed to the analysis of the data in Fig. 2 as follows: write y_{ij} for the difference (observed yield in lower plot) − (observed yield in upper plot) of the (i, j) pair. It will be noted that the $8st$ values of y_{ij} will fall into two sets:

Set (α), say, containing the $4st$ values which have been obtained by subtracting the yield of variety B from that of variety A. This will include from Table II y_{12}, y_{13}, y_{21} and y_{24}.

Set (β), containing the remaining values, for which the yield of A has been subtracted from that of B; e.g. y_{11}, y_{14}, y_{22} and y_{23} in Table II.

The $8st$ differences of observed yields may be set out in the following scheme:

TABLE II

	1st column of pairs	2nd column of pairs	3rd column of pairs	4th column of pairs		Expectation of total
1st row of pairs	$y_{11} = F_{..} + R_1 + C_1 - \Delta + u_{11}$	$y_{12} = F_{..} + R_1 + C_2 + \Delta + u_{12}$	$y_{13} = F_{..} + R_1 + C_3 + \Delta + u_{13}$	$y_{14} = F_{..} + R_1 + C_4 - \Delta + u_{14}$		$4t\,(F_{..} + R_1)$
2nd row of pairs	$y_{21} = F_{..} + R_2 + C_1 + \Delta + u_{21}$	$y_{22} = F_{..} + R_2 + C_2 - \Delta + u_{22}$	$y_{23} = F_{..} + R_2 + C_3 - \Delta + u_{23}$	$y_{24} = F_{..} + R_2 + C_4 + \Delta + u_{24}$	etc.	$4t\,(F_{..} + R_2)$
etc.						
Expectation of totals	$2s\,(F_{..} + C_1)$	$2s\,(F_{..} + C_2)$	$2s\,(F_{..} + C_3)$	$2s\,(F_{..} + C_4)$		$8st\,F_{..}$

Here the u_{ij} are the differences of the ϵ's referred to above, and are supposed normally and randomly distributed about zero with unknown variance σ^2. We shall now write $y_{..}$ for the grand mean of the y's, $y_{i.}$ for the mean y in the ith row, $y_{.j}$ for the mean y in the jth column, $y'_{..}$ for the mean of the $4st$ y's of set (α) and $y''_{..}$ for the mean of the $4st$ y's of set (β). Notice that

$$y_{..} = \tfrac{1}{2}(y'_{..} + y''_{..}), \quad \tfrac{1}{2}(y'_{..} - y''_{..}) = y_{..} - y''_{..} = -(y_{..} - y'_{..}).$$

With these definitions it may be shown by application either of the Markoff theorem or the usual procedure for testing linear hypotheses, that

(a) $y_{..}$ is an unbiased estimate of $F_{..}$

(b) $y_{i.} - y_{..}$ is an unbiased estimate of R_i

(c) $y_{.j} - y_{..}$ is an unbiased estimate of C_j

(d) $\tfrac{1}{2}(y'_{..} - y''_{..})$ is an unbiased estimate of Δ.

(e) The differences, for set (α),

$$y_{ij} - y_{..} - (y_{i.} - y_{..}) - (y_{.j} - y_{..}) - \tfrac{1}{2}(y'_{..} - y''_{..}) = y_{ij} - y_{i.} - y_{.j} + y''_{..},$$

for set (β)
$$y_{ij} - y_{..} - (y_{i.} - y_{..}) - (y_{.j} - y_{..}) + \tfrac{1}{2}(y'_{..} - y''_{..}) = y_{ij} - y_{i.} - y_{.j} + y'_{..},$$

are normally distributed about zero, and if we write the sums of squares

$$S_0^2 = \sum_i^{\alpha}\sum_j (y_{ij} - y_{i.} - y_{.j} + y''_{..})^2 + \sum_i^{\beta}\sum_j (y_{ij} - y_{i.} - y_{.j} + y'_{..})^2, \qquad \text{.....(14)}$$

then the expected value, $$E(S_0^2) = (8st - 2s - 4t)\sigma^2. \qquad \text{.....(15)}$$

Hence we obtain the following partition of the total sum of squares:

$$\sum_j\sum_i (y_{ij}^2) = 8sty_{..}^2 + 4t\sum_i (y_{i.} - y_{..})^2 + 2s\sum_j (y_{.j} - y_{..})^2 + 8st\left(\frac{y'_{..} - y''_{..}}{2}\right)^2 + S_0^2. \qquad \text{.....(16)}$$

Degrees of freedom:

$$8st = 1 + (2s - 1) + (4t - 1) + 1 + (8st - 2s - 4t).$$

Applying this theory to the 96 differences obtained by subtracting the upper plot from the lower plot value for all the pairs of Fig. 2, and remembering that $s = 4$, $t = 3$, we reach the following analysis of variance table:

TABLE III

Sum of squares due to		Degrees of freedom	Mean square
Average fertility slope for whole field	157221	1	
Vertical (row) fertility slopes	4351285	7	
Horizontal (column) fertility slopes	373246	11	
Varietal difference	2741	1	
Residual	4502606	76	59245
Total	9387099	96	

The estimate of σ obtained from the residual is therefore 243·4. The total treatment difference is $48(y'_{..} - y''_{..}) = 513$ g., which has an estimated standard error of

$$\sqrt{(96)} \times 243{\cdot}4 = 2385 \text{ g.}$$

"Student's" point that the difference is well within the standard error is therefore established.

On comparing Table III with that given by "Student" on p. 371 above, it will be seen that the sum of our 1st and 2nd row is equal to his 2nd row, while our 4th row agrees with his 3rd row. The method he has used for extracting the horizontal fertility slopes was however probably not justifiable, and consequently his residual sum of squares is too small, as also his estimate (2259) of the standard error of the total treatment difference. In stating on p. 371 and also in the introductory remarks on p. 363 that a balanced arrangement gives "a slightly smaller error" than the randomised one he appears to have been at fault; these points, however, he would have undoubtedly cleared up before the paper was printed.

We may conclude this note by considering the consequences of a disagreement between the theoretical set-up concerning the level of fertility and what happens in practice. If the theoretical set-up is ideally correct, then any of the expressions

$$y_{ij} - y_{i.} - y_{.j} + y''_{..} \quad \text{or} \quad y_{ij} - y_{i.} - y_{.j} + y'_{..}, \qquad \ldots\ldots(17)$$

will vary about zero with their standard deviations equal to $\sigma\sqrt{\{(8st - 2s - 4t)/8st\}}$. If, however, the theoretical model fails to represent the actual fertility changes in the field, then some at least of the expressions (17) will vary about means m_{ij}, say, different from zero. Consequently the expected value of the S_0^2 of (14) could be broken up into two components, one of which will be the estimate of σ^2 multiplied by the degrees of freedom, and the other, always positive, will be proportional to $\Sigma\Sigma(m_{ij}^2)$. Thus if the model of the soil fertility is not correct, which strictly speaking will always be the case, the expression for S_0^2 will tend to over-estimate the actual error variance. This effect seems to have been what "Student" had in mind. How large or how frequent such over-estimations may be it is impossible to say, except by special inquiry on numerous uniformity trial data.

Similar considerations apply to the effect of the discrepancy between the mathematical model and the actual changes in fertility level on the estimate of the mean difference Δ. But here we may notice that whatever could be said against the chances of errors due to changes in soil fertility cancelling out in the estimate $\frac{1}{2}(y'_{..} - y''_{..})$ calculated from "Student's" suggested lay-out, could be applied with as much or more emphasis to many other experimental arrangements, such as for example the Latin Square.

There remains, of course, the question raised by "Student" himself as to whether and in what cases this new lay-out would be practical.

THE FIRST SIX MOMENTS OF χ^2 FOR AN n-FOLD TABLE WITH n DEGREES OF FREEDOM WHEN SOME EXPECTATIONS ARE SMALL

By J. B. S. HALDANE

HALDANE (1937) gave the first four moments of this distribution. They were derived as a special case of a more general formula. They may, however, be derived directly, by a simple process which allows of the calculation of higher moments with relative ease.

Consider a large sample, in which the number of individuals of a certain type, or the number of expected successes if all the experiments in the sample are independent, is m, where m is small compared with the number in the sample. Then if x be the observed number of individuals of the type considered, or the number of successes in the sample, the probability of observing x is $\frac{m^x}{e^m x!}$, the probabilities forming a Poisson series. It can further readily be shown that the moment-generating function of such a series, moments being taken about zero, is $e^{m(e^t-1)}$. If moments are taken about the mean m, this function is $e^{m(e^t-1-t)}$. The cumulant-generating function is therefore $m(e^t-1)$, and all cumulants are equal to m. The moments about the mean are most readily calculated from the cumulants by equations which have been given by Fisher (1928) up to μ_8, and are continued below up to μ_{12}. These equations, which hold for any distribution, are as follows for the even moments, which alone concern us:

$$\mu_2 = \kappa_2,$$

$$\mu_4 = \kappa_4 + 3\kappa_2^2,$$

$$\mu_6 = \kappa_6 + 5(3\kappa_4\kappa_2 + 2\kappa_3^2) + 15\kappa_2^3,$$

$$\mu_8 = \kappa_8 + 7(4\kappa_6\kappa_2 + 8\kappa_5\kappa_3 + 5\kappa_4^2) + 70(3\kappa_4\kappa_2^2 + 4\kappa_3^2\kappa_2) + 105\kappa_2^4,$$

$$\begin{aligned}\mu_{10} = \kappa_{10} &+ 3(15\kappa_8\kappa_2 + 40\kappa_7\kappa_3 + 70\kappa_6\kappa_4 + 42\kappa_5^2) \\ &+ 105(6\kappa_6\kappa_2^2 + 24\kappa_5\kappa_3\kappa_2 + 15\kappa_4^2\kappa_2 + 20\kappa_4\kappa_3^2) \\ &+ 3150(\kappa_4\kappa_2^3 + 2\kappa_3^2\kappa_2^2) + 945\kappa_2^5,\end{aligned}$$

$$\begin{aligned}\mu_{12} = \kappa_{12} &+ 11(6\kappa_{10}\kappa_2 + 20\kappa_9\kappa_3 + 45\kappa_8\kappa_4 + 72\kappa_7\kappa_5 + 42\kappa_6^2) \\ &+ 33(45\kappa_8\kappa_2^2 + 240\kappa_7\kappa_3\kappa_2 + 420\kappa_6\kappa_4\kappa_2 + 280\kappa_6\kappa_3^2 + 252\kappa_5^2\kappa_2 \\ &\qquad + 840\kappa_5\kappa_4\kappa_3 + 175\kappa_4^3) \\ &+ 385(36\kappa_6\kappa_2^3 + 216\kappa_5\kappa_3\kappa_2^2 + 135\kappa_4^2\kappa_2^2 + 360\kappa_4\kappa_3^2\kappa_2 + 40\kappa_3^4) \\ &+ 17{,}325(3\kappa_4\kappa_2^4 + 8\kappa_3^2\kappa_2^3) + 10{,}395\,\kappa_2^6.\end{aligned}$$

In these equations the coefficient of $\kappa_a^\alpha \kappa_b^\beta \kappa_c^\gamma \ldots$ is

$$\frac{(a\alpha+b\beta+c\gamma+\ldots)!}{\alpha!\,\beta!\,\gamma!\ldots(a!)^\alpha(b!)^\beta(c!)^\gamma\ldots}.$$

Terms bracketed together are multiples of the same power of m in the case of the Poisson distribution and therefore

$$\mu_2 = m,$$
$$\mu_4 = m + 3m^2,$$
$$\mu_6 = m + 25m^2 + 15m^3,$$
$$\mu_8 = m + 119m^2 + 490m^3 + 105m^4,$$
$$\mu_{10} = m + 501m^2 + 6{,}825m^3 + 9{,}450m^4 + 945m^5,$$
$$\mu_{12} = m + 2{,}035m^2 + 74{,}316m^3 + 302{,}995m^4 + 190{,}575m^5 + 10{,}395m^6.$$

These are the even moments of $(x-m)$ about its mean, zero. They are therefore the successive moments of $(x-m)^2$ about zero, which is not its mean. The means of powers, or moments about zero, of $\chi^2 = \dfrac{(x-m)^2}{m}$, for a single degree of freedom, are therefore

$$\mu_1' = 1,$$
$$\mu_2' = 3 + m^{-1},$$
$$\mu_3' = 15 + 25m^{-1} + m^{-2},$$
$$\mu_4' = 105 + 490m^{-1} + 119m^{-2} + m^{-3},$$
$$\mu_5' = 945 + 9{,}450m^{-1} + 6{,}825m^{-2} + 501m^{-3} + m^{-4},$$
$$\mu_6' = 10{,}395 + 190{,}575m^{-1} + 302{,}995m^{-2} + 74{,}316m^{-3} + 2{,}035m^{-4} + m^{-5}.$$

The moments about the mean, 1, are

$$\mu_2 = 2 + m^{-1},$$
$$\mu_3 = 8 + 22m^{-1} + m^{-2},$$
$$\mu_4 = 60 + 396m^{-1} + 115m^{-2} + m^{-3},$$
$$\mu_5 = 544 + 7{,}240m^{-1} + 6{,}240m^{-2} + 496m^{-3} + m^{-4},$$
$$\mu_6 = 6{,}040 + 140{,}740m^{-1} + 263{,}810m^{-2} + 71{,}325m^{-3} + 2{,}029m^{-4} + m^{-5}.$$

The cumulants are

$$\kappa_1 = 1,$$
$$\kappa_2 = 2 + m^{-1},$$
$$\kappa_3 = 8 + 22m^{-1} + m^{-2},$$
$$\kappa_4 = 48 + 384m^{-1} + 112m^{-2} + m^{-3},$$
$$\kappa_5 = 384 + 6{,}720m^{-1} + 6{,}000m^{-2} + 486m^{-3} + m^{-4},$$
$$\kappa_6 = 3{,}840 + 21{,}300m^{-1} + 249{,}600m^{-2} + 69{,}160m^{-3} + 2{,}004m^{-4} + m^{-5}.$$

Hence if we have a series of n samples, in the rth of which the expectation is m_r, and if $R_s = \sum_{r=1}^{n} m_r^{-s}$, then the cumulants of $\chi^2 = \sum_{r=1}^{n} (x_r - m_r)^2/m_r$ are

$$\left.\begin{aligned}
\kappa_1 &= n,\\
\kappa_2 &= 2n + R_1,\\
\kappa_3 &= 8n + 22R_1 + R_2,\\
\kappa_4 &= 48n + 384R_1 + 112R_2 + R_3,\\
\kappa_5 &= 384n + 6720R_1 + 6{,}000R_2 + 486R_3 + R_4,\\
\kappa_6 &= 3{,}840n + 21{,}300R_1 + 249{,}600R_2 + 69{,}160R_3 + 2{,}004R_4 + R_5.
\end{aligned}\right\} \quad \ldots\ldots(1)$$

The successive moments are therefore

$$\begin{aligned}
\mu_2 &= 2n + R_1,\\
\mu_3 &= 8n + 22R_1 + R_2,\\
\mu_4 &= 12n(n+4) + 12(n+32)R_1 + R_1^2 + 112R_2 + R_3,\\
\mu_5 &= \kappa_5 + 10\kappa_3\kappa_2,\\
\mu_6 &= \kappa_6 + 15\kappa_4\kappa_2 + 10\kappa_3^2 + 15\kappa_2^3.
\end{aligned}$$

The higher moments are very considerably increased, even when m is as large as 5. In this case

μ_2 is increased from $2n$ to $2{\cdot}2n$,

μ_3 ,, $8n$ to $12{\cdot}44n$,

μ_4 ,, $12n^2 + 48n$ to $14{\cdot}44n^2 + 129{\cdot}288n$,

μ_5 ,, $160n^2 + 384n$ to $273{\cdot}68n^2 + 1{,}971{\cdot}89n$,

μ_6 ,, $120n^3 + 2{,}080n^2 + 3{,}840n$ to $159{\cdot}72n^3 + 5{,}814{\cdot}04n^2 + 18{,}640{\cdot}49n$.

It will, however, be noticed that when n is large the corrections are relatively small, since the leading term is a multiple of $\kappa_2^{\frac{1}{2}r}$ or $\kappa_2^{\frac{1}{2}(r-1)}\kappa_3$, and the first two cumulants are less affected than the later ones. The coefficients in equations (1) occur in the expressions for the cumulants of χ^2 in other cases, and may therefore be used as checks on any calculations of them.

It will be noticed that the new distribution deviates further from normality than the χ^2 distribution for large expectations. If $\gamma_1 = (\beta_1)^{\frac{1}{2}} = \mu_3\mu_2^{-\frac{3}{2}}$ and

$$\gamma_2 = \beta_2 - 3 = \mu_4\mu_2^{-2} - 3$$

are the measures of deviation when expectations are large, and γ_1', γ_2' the same when expectations are small, then $\frac{\gamma_1'}{\gamma_1}$ approximates to $1 + 2R_1n^{-1}$, and $\frac{\gamma_2'}{\gamma_2}$ approximates to $1 + 7R_1n^{-1}$.

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THE APPROXIMATE NORMALIZATION OF A CLASS OF FREQUENCY DISTRIBUTIONS

By J. B. S. HALDANE

One of the central problems of practical statistics is to determine the probability that the divergence of a variable from its expected value should be due to sampling error. For this purpose we require, in general, a table of the integral, or of the sum between certain limits, of the distribution function of the variable in question.

Such tables have been drawn up for the normal distribution and for a certain number of others. When the frequency distribution depends on a single arbitrary parameter, and especially when, as with the χ^2 distribution, this parameter is an integer, tabulation is not very difficult. But where several arbitrary parameters are concerned it is very tedious, and where the number of arbitrary parameters is large, it is quite impracticable. Such a case arises with the χ^2 distribution when expectations are small (Haldane, 1937).

It has, however, long been known that in many cases the distribution of a statistic derived from a sample of n members of a population, or from an experiment repeated n times, tends to normality when n is large. The deviations from normality can sometimes be conveniently expressed by means of Hermitian polynomials. It will be shown that in a large group of cases a simple transformation of the statistic causes its distribution to approximate very much more rapidly than before to normality when n increases.

One of the two transformations here described was first given by Wilson and Hilferty (1931) in the case of χ^2. It will be shown that this transformation may be applied to other distributions as well. And a second transformation, which in some cases is more powerful, will be described. It will be shown that the distribution of the transformed variate often tends so rapidly to normality when n increases that Sheppard's table of the probability integral is entirely sufficient for practical purposes.

Cumulative statistics

An important class of statistics has the following property. If x_m and x_n are the values of the statistic derived from m members of a population (or m experiments) and n different members or experiments, then $x_{m+n}=x_m+x_n$. That is to say x_m is the sum of m independently ascertained values of the variate x. Let $df=f(x)\,dx$ be the distribution of the variate in the population sampled. Where the distribution of x is wholly or partly discontinuous the appropriate expression can readily be given. And let κ_r be the rth cumulant or semi-invariant of this distribution, defined as the coefficient of $\frac{t^r}{r!}$ in the expansion of $\log \int_{-\infty}^{\infty} e^{tx} f(x)\,dx$.

Then if $\kappa_{r,n}$ be the rth cumulant of the distribution of x_n, it can easily be shown that $\kappa_{r,n} = n\kappa_r$.

Statistics with this property may be called cumulative statistics. The best known examples of them are (i) the number of successes in n trials when the probability of success is constant, and (ii) χ^2. They have the property that all their cumulants tend to infinity with n, though the moments after μ_3 tend to infinity with higher powers of n.

Another important class may be called derived cumulative statistics. They have the property that when multiplied by n, or some power of n, all their non-zero cumulants tend to infinity with n. Thus the mean of a sample clearly has the property, and the cumulants of n times the second moment of a sample of n are

$$(n-1)\,\kappa_2; \quad 2\,(n-1)\,\kappa_2^2 + \frac{(n-1)^2}{n}\kappa_4;$$

$$8\,(n-1)\,\kappa_2^3 + \frac{4\,(n-1)\,(n-2)}{n}\kappa_3^2 + \frac{12\,(n-1)^2}{n}\kappa_2\kappa_4 + \frac{(n-1)^3}{n^2}\kappa_6;$$

and so on, all of which tend to infinity with n. When the expected value of a derived cumulative statistic is constant, its rth cumulant tends to zero with n^{1-r}. The transformations here described are applicable to cumulative and derived cumulative statistics provided that their distribution is not symmetrical, or more accurately provided κ_3 does not vanish.

WILSON AND HILFERTY'S TRANSFORMATION OF χ^2

The rth cumulant of the χ^2 distribution for n degrees of freedom is $n\,(r-1)\,!\,2^{r-1}$. So the first six cumulants are: $\kappa_1 = n$, $\kappa_2 = 2n$, $\kappa_3 = 8n$, $\kappa_4 = 48n$, $\kappa_5 = 384n$, $\kappa_6 = 3840n$, and the corresponding moments about the mean, n, are:

$$\mu_2 = 2n,\ \mu_3 = 8n,\ \mu_4 = 12n\,(n+4),\ \mu_5 = 32n\,(5n+12),\ \mu_6 = 40n\,(3n^2+52n+96).$$

With Wilson and Hilferty we put $\chi^2 = n + x$, and investigate the distribution of $y = \left(\frac{\chi^2}{n}\right)^h$, where h is a constant to be chosen so as to give an approximately normal distribution. We find

$$y = \left(1+\frac{x}{n}\right)^h$$

$$= 1 + \frac{hx}{n} + h\,(h-1)\,\frac{x^2}{2!\,n^2} + h\,(h-1)\,(h-2)\,\frac{x^3}{3!\,n^3} + \ldots,$$

which converges provided $|\,x\,| < n$.

But the mean value of x^r is μ_r, the rth moment of χ^2 about its mean.

Hence

$$\bar{y} = 1 + h(h-1)\frac{\mu_2}{2!\,n^2} + h(h-1)(h-2)\frac{\mu_3}{3!\,n^3} + \dots$$

$$= 1 + h(h-1)\frac{1}{n} + h(h-1)(h-2)\frac{4}{3n^2} + h(h-1)(h-2)(h-3)\frac{(n+4)}{2n^3}$$

$$+ h(h-1)(h-2)(h-3)(h-4)\frac{4n(5n+12)}{15n^4}$$

$$+ h(h-1)(h-2)(h-3)(h-4)(h-5)\frac{(3n^2+52n+96)}{18n^5} + O(n^{-4})$$

$$= 1 + h(h-1)n^{-1} + \tfrac{1}{6}h(h-1)(h-2)(3h-1)n^{-2}$$
$$+ \tfrac{1}{6}h^2(h-1)^2(h-2)(h-3)n^{-3} + O(n^{-4}) \qquad \dots\dots(1)$$

It may be remarked that this series is always convergent when n is sufficiently large, since μ_r is of order $n^{\frac{1}{2}r}$ when r is even, and $n^{\frac{1}{2}(r-1)}$ when n is odd.

Wilson and Hilferty (1931, p. 686, last line) give an expression which, putting their $p^{-1} = h$, is equivalent to

$$\bar{y} = 1 + h(h-1)n^{-1} + \tfrac{4}{3}h(h-1)(h-2)n^{-2} + \dots.$$

It would seem that they neglected the contribution of μ_4 to the coefficient of n^{-2}. This does not, however, affect the validity of their transformation (5). Indeed, it is a better approximation than would appear from a first reading of their paper.

By substituting rh for h in equation (1) we can readily find the mean of y^r as

$$\overline{y^r} = 1 + rh(rh-1)n^{-1} + \tfrac{1}{6}rh(rh-1)(rh-2)(3rh-1)n^{-2} + \dots.$$

Hence we can calculate the moments of y about its mean $\bar{y}$. For example, the third moment is $\mu_3' = 4(3h-1)n^{-2} + O(n^{-3})$.

With Wilson and Hilferty we put $h = \frac{1}{3}$, so that the term of order n^{-2} vanishes, and find

$$\begin{aligned}
\bar{y} &= 1 - 2.3^{-2}n^{-1} + 80.3^{-7}n^{-3} + O(n^{-4}),\\
\overline{y^2} &= 1 - 2.3^{-2}n^{-1} + 4.3^{-4}n^{-2} + 56.3^{-7}n^{-3} + O(n^{-4}),\\
\overline{y^3} &= 1,\\
\overline{y^4} &= 1 + 4.3^{-2}n^{-1} - 4.3^{-3}n^{-2} + 80.3^{-7}n^{-3} + O(n^{-4}).
\end{aligned}$$

Hence

$$\left.\begin{aligned}
\text{Mean} = \kappa_1' &= 1 - 2.3^{-2}n^{-1} + 80.3^{-7}n^{-3} + O(n^{-4}),\\
\mu_2' = \kappa_2' &= 2.3^{-2}n^{-1} - 104.3^{-7}n^{-3} + O(n^{-4}),\\
\mu_3' = \kappa_3' &= 32.3^{-6}n^{-3} + O(n^{-4}),\\
\mu_4' &= 4.3^{-3}n^{-2} - 16.3^{-6}n^{-3} + O(n^{-4}),\\
\kappa_4' &= 16.3^{-6}n^{-3} + O(n^{-4}).
\end{aligned}\right\} \qquad \dots\dots(2)$$

Here and throughout we shall use accents to denote moments and cumulants of a transformed distribution. The standard deviation is therefore

$$\sigma' = \frac{1}{3}\sqrt{\frac{2}{n}}\left(1 - \frac{26}{243n^2}\right) + O(n^{-\frac{5}{2}}),$$

and the first two measures of deviation from normality are

$$\begin{aligned}\gamma_1' &= 2^{\frac{1}{2}}\,3^{-3}\,n^{-\frac{1}{2}} + O(n^{-\frac{1}{2}}) \\ &= 0{\cdot}4190\,n^{-\frac{1}{2}} + O(n^{-\frac{1}{2}}), \\ \gamma_2' &= -4.3^{-2}\,n^{-1} + O(n^{-2}) \\ &= -0{\cdot}4444\,n^{-1} + O(n^{-2}).\end{aligned}$$

These measures are the third and fourth cumulants of the distribution reduced by making its standard deviation unity, and vanish in the case of a normal distribution i.e. $\gamma_1' = \sqrt{\beta_1} = \mu_3'(\mu_2')^{-\frac{3}{2}}$, and $\gamma_2' = \beta_2 - 3 = \kappa_4'(\kappa_2')^{-2}$. They may be compared with the corresponding measures of deviation from normality of the χ^2 distribution itself, namely

$$\begin{aligned}\gamma_1 &= 2^{\frac{1}{2}}\,n^{-\frac{1}{2}} + O(n^{-\frac{1}{2}}) \\ &= 2{\cdot}828\,n^{-\frac{1}{2}} + O(n^{-\frac{1}{2}}),\end{aligned}$$

and

$$\gamma_2 = 12\,n^{-1} + O(n^{-2}).$$

The transformation thus not only reduces γ_1 by a factor $4/(27n)$, but also reduces $|\gamma_2|$ by a factor $1/27$. For this reason it produces a better approximation to normality, even for small values of n, than might have been expected. Similar transformations of other distributions will not necessarily reduce γ_2. Wilson and Hilferty's table shows that, even for $n = 2$, the values of χ^2 in the neighbourhood of $P = 0{\cdot}05$ and $P = 0{\cdot}01$ given by taking the terms of order n^{-1} only for the mean and variance are correct within 1 %. A convenient expression of their result is to say that the variable

$$\xi = \left[\left(\frac{\chi^2}{n}\right)^{\frac{1}{3}} + \frac{2}{9n} - 1\right]\left(\frac{9n}{2}\right)^{\frac{1}{2}} \qquad \text{.....(3)}$$

is almost normally distributed with mean zero and standard deviation unity. This variable has been used in genetics by de Winton and Haldane (1935). However since the distribution of χ^2 has already been tabulated for values of n up to 30 it is unlikely that equation (3) will be much used in practice.

GENERALIZATION OF WILSON AND HILFERTY'S THEOREM.

Consider a variable so distributed that, when n tends to infinity, the orders of magnitude of its cumulants are as follows:

$$\kappa_1 = O(n^{1+a}),\ \kappa_2 = O(n^{1+2a}),\ \kappa_3 = O(n^{1+3a}),\ \kappa_4 \leqslant O(n^{1+4a}),$$

and for higher values of r, $\kappa_r \leqslant O(n^{r+ra-4})$, where a is any constant. The departures from normality, if any, are not affected by multiplying the variable by a constant. So multiplying by n^{-a} we obtain a variate so distributed that $\kappa_1 = \kappa_2 = \kappa_3 = O(n)$, $\kappa_4 \leqslant O(n)$, $\kappa_r \leqslant O(n^{r-4})$. In what follows we shall consider the substitution as made.

Our variates include most cumulative and derived cumulative statistics, and some others. Since however cases where $\kappa_3 = 0$ are excluded, we cannot deal with symmetrical distributions, such as "Student's" (1908) distribution of the mean of a sample in terms of its standard deviation, or Fisher's (1928) distribution of the estimate of γ_1. It will also be noted that a number of distributions which tend to normality with n are excluded, for example that of the transformed correlation coefficient.

Let $$x = \kappa_1 + x', \quad \text{and} \quad y = \left(\frac{x}{\kappa_1}\right)^h.$$

Then $$y = \left(1 + \frac{x'}{\kappa_1}\right)^h = 1 + h\,\frac{x'}{\kappa_1} + h(h-1)\,\frac{x'^2}{2!\,\kappa_1^2} + \ldots.$$

Writing $rh(rh-1)(rh-2)\ldots(rh-n+1) = f(r,n)$, we have

$$\overline{y^r} = 1 + f(r,2)\,\frac{\mu_2}{2!\,\kappa_1^2} + f(r,3)\,\frac{\mu_3}{3!\,\kappa_1^3} + f(r,4)\,\frac{\mu_4}{4!\,\kappa_1^4} + \ldots,$$

since μ_r, the rth moment of x about its mean, is the mean of x'^r. Hence

$$\overline{y^r} = 1 + f(r,2)\,\frac{\kappa_2}{2\kappa_1^2} + f(r,3)\,\frac{\kappa_3}{6\kappa_1^3} + f(r,4)\,\frac{(3\kappa_2^2 + \kappa_4)}{24\kappa_1^4}$$

$$+ f(r,5)\,\frac{\kappa_2\kappa_3}{12\kappa_1^5} + f(r,6)\,\frac{\kappa_2^3}{48\kappa_1^6} + O(n^{-4}). \qquad \ldots\ldots(4)$$

Since the sum of the suffixes of the cumulants in the numerator is always equal to the power of κ_1 in the denominator it is clear that in this expression factors of the form n^{ra} will cancel out. The term of highest order in which κ_s appears is a multiple of $\kappa_s\kappa_1^{-s}$, which, if $s > 4$ is of order n^{-4} or less, and therefore does not enter into equation (4). The series converges if n is large enough.

It is clear that we can calculate the moments of y about its mean, and choose h, except in one special case, so that the leading term of the third moment vanishes. We can therefore enunciate the following theorem:

"If a variate x be so distributed that its first three cumulants tend to infinity

with n, the fourth with n or more slowly, and in general κ_r with n^{r-4} or more slowly; then if $h = 1 - \frac{\kappa_1\kappa_3}{3\kappa_2^2}$, $y = \left(\frac{x}{\kappa_1}\right)^h$ is so distributed that its third moment tends to zero with n^{-3}, and hence its γ_1 tends to zero with $n^{-\frac{1}{2}}$."

The statement remains true if x be multiplied by any constant, such as a power of n. But the theorem clearly breaks down if $\kappa_1\kappa_3 = 3\kappa_2^2$. In this case however the transformation can be performed after adding a suitable constant of order n to the variate. If h has a negative value this may be adjusted by altering the mean as above. Otherwise the approximation (7) given below will clearly break down when x is very small or negative. But this implies that x exceeds its standard deviation $\kappa_2^{\frac{1}{2}}$ by a factor of $|\kappa_1|\kappa_2^{-\frac{1}{2}}$, which is of order $n^{\frac{1}{2}}$. When n is sufficiently large such deviations will be excessively unlikely, and approximation (7) may prove to be valid even when h is negative.

The proof follows. By substituting appropriate values of r in equation (4) we can readily deduce the mean and the first few moments of y. The algebra is tedious but elementary. For example the coefficient of $\frac{3\kappa_2+\kappa_4}{24\kappa_1^3}$ in the expression for μ_4' is:

$$f(4,4) + 6f(2,4) - 4f(1,4) - 4f(3,4) = 24h^4.$$

In the expression for μ_4' the term of order n^{-4} is given, on the assumption that κ_4 and κ_5 are of order n, and throughout, terms of the same order of magnitude are grouped together. The mean and first few moments about the mean of y are

$$\left.\begin{aligned}
\text{Mean} = \kappa_1' &= 1 + h(h-1)\frac{\kappa_2}{2\kappa_1^2} + h(h-1)(h-2)\frac{[4\kappa_1\kappa_3 + 3(h-3)\kappa_2^2]}{24\kappa_1^4} \\
&\quad + h(h-1)(h-2)(h-3)\frac{[2\kappa_1^2\kappa_4 + 4(h-4)\kappa_1\kappa_2\kappa_3 + (h-4)(h-5)\kappa_2^3]}{48\kappa_1^6} + O(n^{-4}), \\
\mu_2' = \kappa_2' &= \frac{h^2\kappa_2}{\kappa_1^2} + h^2(h-1)\frac{[2\kappa_1\kappa_3 + (3h-5)\kappa_2^2]}{2\kappa_1^4} \\
&\quad + h^2(h-1)\frac{[(7h-11)\kappa_1^2\kappa_4 + 4(h-2)(7h-12)\kappa_1\kappa_2\kappa_3 + 2(h-2)(7h^2-30h+32)\kappa_2^3]}{12\kappa_1^6} \\
&\quad + O(n^{-4}), \\
\mu_3' = \kappa_3' &= \frac{h^3[\kappa_1\kappa_3 + 3(h-1)\kappa_2^2]}{\kappa_1^4} \\
&\quad + h^3(h-1)\frac{[3\kappa_1^2\kappa_4 + 3(7h-10)\kappa_1\kappa_2\kappa_3 + (17h^2-55h+44)\kappa_2^3]}{2\kappa_1^6} + O(n^{-4}), \\
\mu_4' &= \frac{h^4(h-1)}{4(\kappa_1^8)}\left[\begin{aligned}
&8\kappa_1^3\kappa_5 + 12(7h-9)\kappa_1^2\kappa_3^2 + 2(59h-79)\kappa_1^3\kappa_2\kappa_4 \\
&+ 4(149h^2-442h+327)\kappa_1\kappa_2\kappa_3 \\
&+ (339h^3-1649h^2+2673h-1447)\kappa_2^4
\end{aligned}\right] + O(n^{-5}).
\end{aligned}\right\} \quad \ldots\ldots(5)$$

If we put $h = 1 - \frac{\kappa_1 \kappa_3}{3\kappa_2^2}$ the leading term of μ_3' vanishes. We now find for the first moments and cumulants of y

$$\left.\begin{aligned}
&\text{Mean} = \kappa_1' = 1 - h(1-h)\frac{\kappa_2}{2\kappa_1^2} + h(1-h)(2-h)(1-3h)\frac{\kappa_2^2}{8\kappa_1^4} + O(n^{-3}),\\
&\mu_2' = \kappa_2' = h^2\frac{\kappa_2}{\kappa_1^2} - h^2(1-h)(1-3h)\frac{\kappa_2^2}{2\kappa_1^4} + O(n^{-3}),\\
&\mu_3' = \kappa_3' = h^3(1-h)\frac{[2(23-49h+23h^2)\kappa_2^3 - 3\kappa_1^2\kappa_4]}{2\kappa_1^6} + O(n^{-4}),\\
&\mu_4' = 3h^4\frac{\kappa_2^3}{\kappa_1^4} + h^4\frac{[3\kappa_1\kappa_4 - (19-29h)\kappa_2\kappa_3]}{3\kappa_1^5} + O(n^{-4}),\\
&\kappa_4' = \frac{h^4}{3\kappa_1^5}[3\kappa_1\kappa_4 - 4(4-5h)\kappa_2\kappa_3] + O(n^{-4}).
\end{aligned}\right\} \quad \ldots\ldots(6)$$

Since h, κ_1, κ_2, κ_3 are not independent, these expressions can clearly be written in many ways. However the above are probably the simplest. Since the standard deviation of y is

$$\frac{h\kappa_2^{\frac{1}{2}}}{\kappa_1}\left[1 - \frac{(1-h)(1-3h)\kappa_2}{4\kappa_1^2}\right] + O(n^{-\frac{1}{2}})$$

we may conveniently say that

$$\xi = \left[\left(\frac{x}{\kappa_1}\right)^h + \frac{h(1-h)\kappa_2}{2\kappa_1^2}\left\{1 - \frac{(2-h)(1-3h)\kappa_2}{4\kappa_1^2}\right\} - 1\right]\frac{\kappa_1}{h\kappa_2^{\frac{1}{2}}}\left[1 + \frac{(1-h)(1-3h)\kappa_2}{4\kappa_1^2}\right], \quad \ldots\ldots(7)$$

is almost normally distributed with mean zero and standard deviation unity. On substituting a given value of x in this equation we can evaluate ξ and thus obtain the probability of as large or a larger deviation of x from κ_1 by means of Sheppard's table. This transformation will be referred to as transformation A. For large values of n, and particularly when h does not differ greatly from $\frac{1}{3}$, it will be sufficient to put

$$\xi = \left[\left(\frac{x}{\kappa_1}\right)^h + \frac{h(1-h)\kappa_2}{2\kappa_1^2} - 1\right]\frac{\kappa_1}{h\kappa_2^{\frac{1}{2}}}.$$

It follows that the first two measures of deviation of ξ or y from normality are

$$\begin{aligned}
\gamma_1' &= \frac{(1-h)[2(23-49h+23h^2)\kappa_2^3 - 3\kappa_1^2\kappa_4]}{2\kappa_1^3\kappa_2^{\frac{1}{2}}} + O(n^{-\frac{1}{2}})\\
&= \frac{\kappa_3}{54\kappa_1^3\kappa_2^{\frac{1}{2}}}(9\kappa_1\kappa_2^2\kappa_3 + 46\kappa_1^2\kappa_3^2 - 27\kappa_1^2\kappa_2\kappa_4 - 54\kappa_2^3) + O(n^{-\frac{1}{2}}),\\
\gamma_2' &= \frac{3\kappa_1\kappa_4 - 4(4-5h)\kappa_2\kappa_3}{3\kappa_1\kappa_2^2} + O(n^{-2})\\
&= \frac{9\kappa_1\kappa_2\kappa_4 - 20\kappa_1\kappa_3^2 + 12\kappa_2^2\kappa_3}{9\kappa_1\kappa_2^3} + O(n^{-2}).
\end{aligned}$$

Thus γ_1' is of order n^{-1}, as compared with $\gamma_1=\frac{\kappa_3}{\kappa_2^{\frac{3}{2}}}$ of order $n^{-\frac{1}{2}}$, and

$$\frac{\gamma_2'}{\gamma_2}=1-\frac{4(4-5h)\kappa_2\kappa_3}{3\kappa_1\kappa_4}+O(n^{-1})$$

$$=1-\frac{4\kappa_3(5\kappa_1\kappa_3-3\kappa_2^2)}{9\kappa_1\kappa_2\kappa_4}+O(n^{-1}).$$

This may be small numerically. Thus for the χ^2 distribution $\gamma_2'/\gamma_2=-\frac{1}{27}$. When it is not numerically small, a further transformation will reduce it to the order of n^{-1}.

Adjustment of the Fourth Moment

In order that γ_2' should tend to zero with n^{-2}, κ_4' must tend to zero with n^{-4}. This can be secured by adding a suitable constant to x, so that κ_1 assumes any desired value g, whilst the other cumulants are unchanged. In order that the leading term of κ_4' should vanish, we must make $3g\kappa_4-4(4-5h)\kappa_2\kappa_3=0$. Hence

$$g=\frac{12\kappa_2^2\kappa_3}{20\kappa_3^2-9\kappa_2\kappa_4},\quad h=\frac{16\kappa_3^2-9\kappa_2\kappa_4}{20\kappa_3^2-9\kappa_2\kappa_4},$$

whilst for x we must substitute $x+g-\kappa_1$. It is clear that we can now remove our restriction on the order of κ_1 when n is large. On the other hand if κ_2 and κ_3 are of order n, κ_4 must be of the same order. We can now state the following theorem:

"If the variable x be so distributed that its cumulants κ_2, κ_3, κ_4 tend to infinity with r, and no later cumulant tends to infinity more rapidly than n^{r-4}, then if

$$g=\frac{12\kappa_2^2\kappa_3}{20\kappa_3^2-9\kappa_2\kappa_4},\qquad h=\frac{16\kappa_3^2-9\kappa_2\kappa_4}{20\kappa_3^2-9\kappa_2\kappa_4},$$

and

$$z=\left(1+\frac{x-\kappa_1}{g}\right)^h,$$

z is so distributed that its third and fourth cumulants tend to zero with n^{-3} and n^{-4} respectively."

As above, the theorem remains true if x is multiplied by any constant; but it breaks down if $20\kappa_3^2=9\kappa_2\kappa_4$ or $16\kappa_3^2=9\kappa_2\kappa_4$. It follows that for the distribution of z, $\gamma_1''=O(n^{-1})$ and $\gamma_2''=O(n^{-2})$.

To prove the theorem we substitute g for κ_1 in equations (5), and give h its appropriate value. This is equivalent to putting

$$g\kappa_3=3(1-h)\kappa_2^2,$$

$$g^2\kappa_4=4(1-h)(5-4h)\kappa_2^3.$$

The cumulants of z are most simply expressed in terms of

$$b=\frac{h}{g}=\frac{16\kappa_3^2-9\kappa_2\kappa_4}{12\kappa_2^2\kappa_3},\qquad c=\frac{1-h}{g}=\frac{\kappa_3}{3\kappa_2^2}.$$

They are

$$\left.\begin{aligned}
\text{Mean} = \kappa_1'' &= 1 - \tfrac{1}{2}bc\kappa_2 [1 + \tfrac{1}{4}(b+2c)(2b-c)\kappa_2] + O(n^{-3}),\\
\mu_2'' = \kappa_2'' &= b^2\kappa_2 [1 + \tfrac{1}{2}c(2b-c)\kappa_2] + O(n^{-3}),\\
\mu_3'' = \mu_3'' &= -b^3c(3b^2 - 3bc + c^2)\kappa_2^3 + O(n^{-4}),\\
\kappa_4'' &= b^4c(12b^3 + 2b^2c - 138bc^2 + 247c^3)\kappa_2^4 - 2b^4c + O(n^{-5}).
\end{aligned}\right\} \quad \ldots(8)$$

For purposes of calculation it is convenient to write $h = bg$, so that

$$\xi = \left[\left(1 + \frac{x-\kappa_1}{g}\right)^{bg} + \tfrac{1}{2}bc\kappa_2\{1 + \tfrac{1}{4}(b+2c)(2b-c)\} - 1\right]\left[\frac{1 - \tfrac{1}{2}c(2b-c)\kappa_2}{b\kappa_2^{\frac{1}{2}}}\right], \quad \ldots(9)$$

or less accurately

$$\xi = \frac{\left(1 + \dfrac{x-\kappa_1}{g}\right)^{bg} + \tfrac{1}{2}bc\kappa_2 - 1}{b\kappa_2^{\frac{1}{2}}},$$

is almost normally distributed about zero with unit standard deviation and measures of deviation from normality

$$\gamma_1'' = -c(3b^2 - 3bc + c^2)\kappa_2^{\frac{3}{2}} + O(n^{-\frac{1}{2}})$$

$$= \frac{-592\kappa_3^4 + 756\kappa_2\kappa_3^2\kappa_4 - 243\kappa_2^2\kappa_4^2}{432\kappa_2^{\frac{3}{2}}\kappa_3} + O(n^{-\frac{1}{2}}),$$

$$\gamma_2'' = c(12b^3 + 2b^2c - 138bc^2 + 247c^3)\kappa_2^2 - \frac{2c\kappa_5}{\kappa_2^2} + O(n^{-3})$$

$$= \frac{5908\kappa_3^6 - 16344\kappa_2\kappa_3^4\kappa_4 + 11826\kappa_2^2\kappa_3^2\kappa_4^2 + 2187\kappa_2^3\kappa_4^3 - 864\kappa_2^2\kappa_3^2\kappa_5}{1296\kappa_2^6\kappa_3^2} + O(n^{-3}).$$

This transformation will be referred to as transformation B. It may give γ_1 larger or smaller than transformation A. It will certainly give γ_2 smaller for large values of n. However we shall see that in some cases, even when n is as large as 100, transformation A may give a better approximation. h is negative if

$$20\kappa_3^2 > 9\kappa_2\kappa_4 > 16\kappa_3^2.$$

In this case approximation (9) is clearly invalid for negative or very small positive values of $1 + \dfrac{x-\kappa_1}{g}$. It may, however, be found that for sufficiently large values of n such values are very improbable. As will be seen later, in the case of χ^2 this expression cannot become negative, and (in the theory of large samples) cannot vanish.

It is clear that other transformations are possible. If we desired to obtain a highly symmetrical distribution without concerning ourselves with its flatness, we could either choose g so that the term of κ_5' vanished, or we could reduce γ_1' to the order $n^{-\frac{1}{2}}$ by making the leading term in the expression (6) for μ_3' vanish, that is to say by choosing g so that

$$g^2(27\kappa_3\kappa_4 - 46\kappa_3^3) - 9g\kappa_2^2\kappa_3 + 54\kappa_2^3\kappa_4 = 0.$$

Or for a given value of n we could obtain κ_3' and κ_4' in equations (5) as functions of g and h, and choose g and h so that both vanish. For small values of n this would involve the calculation of many terms in the expansions of κ_3' and κ_4' and would rarely be worth doing. It will be seen that in some cases transformation A is fully sufficient for practical purposes.

APPLICATION TO THE χ^2 DISTRIBUTION

For the χ^2 distribution $\kappa_1=n$, $\kappa_2=2n$, $\kappa_3=8n$, $\kappa_4=48n$, $\kappa_5=384n$. Transformation A is Wilson and Hilferty's transformation, whose very satisfactory fit is shown in their paper. In transformation B,

$$g=\frac{12n}{13}, \quad b=\frac{5}{12n}, \quad c=\frac{2}{3n}, \quad h=\frac{5}{13}.$$

So
$$\xi''=\left[\left(\frac{13x^2-n}{12n}\right)^{\frac{5}{13}}+\frac{5}{18n}\left(1+\frac{7}{48n}\right)-1\right]\frac{6\sqrt{2n}}{5}\left(1-\frac{1}{18n}\right) \quad \text{.....(10)}$$

is almost normally distributed about zero with unit standard deviation, and

$$\gamma_1''=\frac{19}{27(2n)^{\frac{1}{2}}}+O(n^{-\frac{1}{2}}),$$

$$\gamma_2''=\frac{35}{18n^2}+O(n^{-3}).$$

Comparing these with the values found above for transformation A we find

$$\frac{\gamma_1''}{\gamma_1'}=\frac{19}{32}+O(n^{-1}),$$

$$\frac{\gamma_2''}{\gamma_2'}=\frac{35}{4n}+O(n^{-2}).$$

Thus the symmetry is always improved, but for $n<9$, γ_2 is increased. However, for large values of n the fit is considerably improved. Thus for $n=10$ we find, for different values of P, the values of χ^2 given in Table I.

TABLE I

Approximations to the χ^2 distribution

P	0·8	0·5	0·2	0·05	0·01
True value of χ^2	6·179	9·342	13·442	18·307	23·209
Approximation A	6·191	9·349	13·419	18·298	23·246
" B	6·182	9·339	13·430	18·288	23·194

Thus for four out of five values of P, transformation B already gives the better fit, and the error nowhere exceeds 0·1 %. For higher values of n, B is still more decidedly superior. However, A is good enough for all practical purposes, besides being decidedly simpler.

In view of certain criticisms which have recently been made of the utility of fitting frequency curves by means of moments, it is perhaps worth emphasizing the high degree of approximation which, in this case at any rate, is reached by adjusting the first four, or even the first three, moments. Clearly, however, the approximation will not always be so good.

For small values of n we can always obtain a better fit by not neglecting higher powers of n^{-1}. Thus it is obvious that for $n=1$, $(\chi^2)^{\frac{1}{2}}$ has an exactly normal distribution, whilst $(\chi^2)^{\frac{1}{2}}$ is not very normally distributed. In important cases such as $n=3$ (the Maxwellian distribution of velocities of gas molecules) it might conceivably be worth searching for the most accurate possible distribution of the type A or B.

Application to the Binomial Distribution

Consider an experiment repeated n times on each of which the probabilities of success and failure are p and q respectively. If x successes are observed the first cumulants of the distribution are very readily shown to be

$$\kappa_1 = np,$$

$$\kappa_2 = npq,$$

$$\kappa_3 = npq\,(q-p),$$

$$\kappa_4 = npq\,(1-6pq),$$

$$\kappa_5 = npq\,(1-12pq)\,(q-p).$$

The normal approximation may be stated as follows:

$\xi = \dfrac{x-np}{(npq)^{\frac{1}{2}}}$ is almost normally distributed about zero with unit standard deviation, and $\gamma_1 = \dfrac{q-p}{(npq)^{\frac{1}{2}}}$, $\gamma_2 = \dfrac{1-6pq}{npq}$.

We may apply transformation A either to x or to $n-x$. If we apply it to x, we have $h = \dfrac{p+2q}{3q}$, so that

$$\xi' = \left[\frac{\left(\dfrac{x}{np}\right)^{\frac{p+2q}{3q}} - 1}{p+2q} + \frac{p-q}{18npq}\left(1+\frac{4q-p}{12npq}\right)\right] 3\sqrt{(npq)}\left(1+\frac{p-q}{3npq}\right) \quad \text{.....(11)}$$

or less accurately,

$$\xi' = \left[\frac{\left(\frac{x}{np}\right)^{\frac{p+2q}{3q}} - 1}{p+2q} + \frac{p-q}{18npq}\right] 3\sqrt{(npq)},$$

is almost normally distributed about zero with unit standard deviation, and with

$$\gamma_1' = \frac{-(p-q)^2(17+2p)}{54(npq)^{\frac{1}{2}}} + O(n^{-\frac{3}{2}}),$$

$$\gamma_2' = \frac{-11p^2 - 8pq + q^2}{9npq} + O(n^{-2}).$$

$|\gamma_1'|$ is always less than $|\gamma_1|$. γ_2' vanishes when $p = \frac{1}{2}(3\sqrt{(3)} - 5) = \cdot 0981$ and does not exceed $\frac{1}{9}\gamma_2'$ for lower values of p. Also $|\gamma_2'| < |\gamma_2|$ if $p < \frac{1}{26}(16 - \sqrt{(126)})$, or $\cdot 1875$. So for small values of p the transformation is likely to be valuable, and even in the neighbourhood of $\frac{1}{2}$, γ_2' has the moderate value of $2n^{-1}$. If $p < \frac{1}{2}$ we should perform transformation A on $n - x$. The transformation has a very slight effect in the neighbourhood of $p = \frac{1}{2}$, but here the normal approximation is already very close.

Transformation B does not seem to be well suited to the binomial distribution, since both p^2q^2 and $(p-q)^2$ occur in the denominator of γ_2'', and numerical tests show that it is not so efficient.

A few numerical examples, given in Table II, show the degree of accuracy

TABLE II

Approximations to P in the binomial distribution

	True value	Normal	"A"	Gram-Charlier
$p = 0{\cdot}25$, $n = 10$, $x = 5{\cdot}5$	0·9803	0·9851	0·9798	0·9731
$p = 0{\cdot}10$, $n = 100$, $x = 3{\cdot}5$	0·0078	0·0157	0·0085	0·0094
$p = 0{\cdot}10$, $n = 100$, $x = 17{\cdot}5$	0·9900	0·9938	0·9900	0·9877

reached by transformation A. In each case we take a value of x intermediate between two integers. The second column gives the probability that x should exceed this value. The third column gives the probability that a normal deviate should exceed $\frac{x - np}{\sqrt{(npq)}}$. The third column gives the probability that a normal deviate should exceed ξ' calculated from the shorter form of equation (11). The last column gives the same calculation derived from the first two terms of the Gram-Charlier series for the probability when the binomial deviate is expanded

in a series of Hermitian polynomials. The correction to the probability of column 2 is $+\frac{p-q}{6\sqrt{(npq)}}e^{-\frac{1}{2}\xi^2}$, where $\xi=\frac{x-np}{\sqrt{(npq)}}$. It will be seen that in each case transformation A gives the best fit, though further terms in the Gram-Charlier series would doubtless give a still better fit.

Discussion

The approximations here given must obviously be used with caution. Like the normal approximation to the binomial, they can give absurd results. For example finite probabilities are found for negative values of χ^2, though even for $n=5$ the probability of a negative value is less than 10^{-5} according to equation (3), whilst equation (10) holds down to a probability of 2×10^{-4}. When the transformations are applied to any particular distribution it is always possible that series (4) may converge very slowly even for fairly large values of n. This is so for example, in the case of the estimate of β_2. The values of this estimate in the samples of n from a normal population are distributed round 3 with cumulants of order n. But their numerical coefficients increase so rapidly that, on applying transformation B,

$$h=\frac{13}{5},\quad g=\frac{4n}{5},$$

and γ_1'' is greater than γ_1 until n exceeds 110.

The main object of this investigation has been to make it possible to deal with the χ^2 distribution and that of Fisher's (1935) u statistics when expectations are small. This will be done in separate papers.

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MISCELLANEA

A Note on a Form of Tchebycheff's Theorem for Two Variables.

By P. O. BERGE
University of Stockholm

Let $f(x, y)$ be a frequency function with the means

$$\bar{x}=0, \quad \bar{y}=0,$$

and the moments of the second order σ_1^2, σ_2^2, μ_{11}.

We shall try to find a limiting value for the probability, that $|x|<k\sigma_1$, and $|y|<k\sigma_2$, i.e. that the point (x, y) lies within the rectangle R, limited by the straight lines

$$x=\pm k\sigma_1, \quad y=\pm k\sigma_2.$$

We shall use a procedure similar to that used by Prof. Karl Pearson in his paper "On Generalized Tchebycheff Theorems in the mathematical theory of statistics" (*Biometrika*, XII, 284) but confining our attention to the case where only the moments of the first and second order are given.

The function

$$\frac{1}{k^2(1-t^2)}\left[\frac{x^2}{\sigma_1^2}-2t\frac{xy}{\sigma_1\sigma_2}+\frac{y^2}{\sigma_2^2}\right] \qquad \ldots\ldots(1)$$

is, if $t^2\leqslant 1$, not negative and outside the rectangle R, greater than 1.

We get

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty}\frac{1}{k^2(1-t^2)}\left[\frac{x^2}{\sigma_1^2}-2t\frac{xy}{\sigma_1\sigma_2}+\frac{y^2}{\sigma_2^2}\right]f(x,y)\,dx\,dy=\frac{2(1-tr)}{k^2(1-t^2)}\geqslant 1-\iint_{(R)}f(x,y)\,dx\,dy,$$

where the last integral is to be taken over the region inside of the rectangle R, and r is the correlation-coefficient.

Thus

$$\iint_{(R)}f(x,y)\,dx\,dy\geqslant 1-\frac{2(1-tr)}{k^2(1-t^2)}. \qquad \ldots\ldots(2)$$

The expression on the left is the probability that

$$|x|<k\sigma_1 \quad \text{and} \quad |y|<k\sigma_2,$$

and our problem will now be to find a minimum value for

$$\frac{2(1-tr)}{k^2(1-t^2)}.$$

Differentiating this expression with regard to t and equating to zero, we get

$$-r(1-t^2)+2t(1-tr)=0$$

or

$$t=\frac{1}{r}(1\pm\sqrt{1-r^2}).$$

But t must satisfy the condition $t^2\leqslant 1$. We find that this condition is fulfilled if we choose

$$t=\frac{1}{r}(1-\sqrt{1-r^2}). \qquad \ldots\ldots(3)$$

For this value of t we get

$$\frac{2(1-tr)}{k^2(1-t^2)}=\frac{1+\sqrt{1-r^2}}{k^2}.$$

Hence we see that
the probability that $|x|<k\sigma_1$ *and* $|y|<k\sigma_2$ *is greater than or equal to*

$$1-\frac{1+\sqrt{1-r^2}}{k^2}. \qquad \ldots\ldots(4)$$

It is possible to show that the value (4) is the most accurate we can reach, when only the moments of the first and second order are known. Consider a discontinuous frequency function with, (i) the probability

$$\frac{1+\sqrt{1-r^2}}{4k^2}$$

attached to each of the four points

$$\left(k\sigma_1,\ \frac{k\sigma_2(1-\sqrt{1-r^2})}{r}\right),\ \left(\frac{k\sigma_1(1-\sqrt{1-r^2})}{r},\ k\sigma_2\right),$$

$$\left(-\frac{k\sigma_1(1-\sqrt{1-r^2})}{r},\ -k\sigma_2\right),\ \left(-k\sigma_1,\ -\frac{k\sigma_2(1-\sqrt{1-r^2})}{r}\right),$$

—these four points lying on the rectangle R—and (ii) the probability

$$1-\frac{1+\sqrt{1-r^2}}{k^2}$$

attached to origin.

If $\bar{x}'$ and $\bar{y}'$ are the means and $\sigma_1'^2$, $\sigma_2'^2$, μ_{11}' are the moments of the second order of the above-mentioned discontinuous frequency function, we get

$$\bar{x}'=\bar{y}'=0,$$

$$\sigma_1'^2=k^2\sigma_1^2\frac{1+\sqrt{1-r^2}}{2k^2}+k^2\sigma_1^2\frac{(1-\sqrt{1-r^2})^2(1+\sqrt{1-r^2})}{2k^2r^2}=\sigma_1^2,$$

$$\sigma_2'^2=k^2\sigma_2^2\frac{(1-\sqrt{1-r^2})^2}{r^2}\frac{(1+\sqrt{1-r^2})}{2k^2}+k^2\sigma_2^2\frac{1+\sqrt{1-r^2}}{2k^2}=\sigma_2^2,$$

$$\mu_{11}'=4k\sigma_1\frac{k\sigma_2(1-\sqrt{1-r^2})}{r}\ \frac{1+\sqrt{1-r^2}}{4k^2}=r\sigma_1\sigma_2.$$

This frequency function consequently has its means at the origin; its two standard deviations are equal to σ_1 and σ_2 and the correlation-coefficient is equal to r.

This example shows that the value (4) is the best one in the sense, that it is impossible to say that the probability for

$$|x|<k\sigma_1 \quad \text{and} \quad |y|<k\sigma_2$$

is *greater* than

$$1-\frac{1+\sqrt{1-r^2}}{k^2}$$

unless we know higher moments.

Finally, it is of interest to compare certain numerical values of the limiting expression (4) with known values of the integral within the rectangle, R, in the case where the frequency function $f(xy)$ is the Normal Bivariate Surface

$$\phi(x,y)=\frac{1}{2\pi\sqrt{1-r^2}}e^{-\frac{1}{2(1-r^2)}(x^2+y^2-2rxy)}$$

k	$\int_{-k}^{k}\int_{-k}^{k}\phi(x,y)\,dxdy$			$1-\frac{1+\sqrt{1-r^2}}{k^2}$		
	$r=0$	$r=0{\cdot}5$	$r=1{\cdot}0$	$r=0$	$r=0{\cdot}5$	$r=1{\cdot}0$
2·0	0·9111	0·9171	0·9545	0·5000	0·5335	0·7500
2·6	0·9814	0·9823	0·9907	0·7041	0·7240	0·8521

$r=1{\cdot}0$ corresponds to the case of *one* variable.

While the approximation is more accurate for high values of r, it will be seen that for this particular frequency surface the limits given by my theorem are drawn very conservatively.

Note on J. B. S. Haldane's Paper: "The Exact Value of the Moments of the Distribution of χ^2." (Biometrika, XXIX, 133–143.)

By W. G. COCHRAN

In this paper Haldane points out (p. 142) a difference between his results for the mean and variance of χ^2 in a $2 \times n$-fold contingency table when the expectation p is fixed and the results obtained by me in my paper (*Annals of Eugenics*, Vol. VII, part III, p. 211). The difference is that I have $(n-1)$ throughout where Haldane has n. Haldane writes, "my own results would appear to be slightly more accurate than Cochran's", which might, I think, give the impression that both Haldane's results and mine are only approximations. In fact, both results are mathematically exact, the difference between them being one of definition of χ^2. My paper is almost entirely concerned with the distribution of χ^2 when the expectation p is not known. In the results which I gave for the distribution of χ^2 when p is known, I retained the term $S(x-\bar{x})^2$ in the numerator of χ^2 instead of $S(x-np)^2$, to facilitate comparison between this and my other results. Thus my χ^2 has $(n-1)$ degrees of freedom, whereas Haldane's χ^2 has n degrees of freedom.

Unfortunately I did not emphasize this point in the passage concerned, and as it may have appeared misleading to others besides Haldane, I welcome this opportunity of drawing attention to it. Haldane's χ^2 is, of course, the one which is normally appropriate in testing the departure from independence when the expectation is known.

The reader may perhaps also wonder why in § 3 I have replaced $\frac{\bar{x}}{n}(n-\bar{x})$ by κ_2 in the denominator of χ^2. This was done because I am discussing the distribution of χ^2 in arrays in which the mean of the sample is equal to the mean of the population, so that in this paragraph the two expressions can be regarded as equivalent.

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CONTENTS

PAGE

Karl Pearson: an appreciation of some aspects of his life and work. By E. S. PEARSON. With six Plates in the Text . . . . . 161—248

Bright's disease, nephritis and arterio-sclerosis: a contribution to the history of medical statistics. By MAJOR GREENWOOD and W. T. RUSSELL . . . . . 249—276

The London skull. By MATTHEW YOUNG. With seven Figures and three Plates in the Text . . . . 277—321

Significance tests which may be applied to samples from any populations. III. The analysis of variance test. By E. J. G. PITMAN . . . . . 322—335

Absence and recovery recorded at irregular intervals, illustrated on school data. By FRANK SANDON. With two Figures in the Text . . . . . 336—349

The significance of the difference between two means when the population variances are unequal. By B. L. WELCH. With a Note by ELIZABETH TANBURN and two Figures in the Text . . . . . 350—362

Comparison between balanced and random arrangements of field plots. By "STUDENT" . . . . . 363—379

Note on some points in "Student's" paper on "Comparison between balanced and random arrangements of field plots". By J. NEYMAN and E. S. PEARSON. With two Figures in the Text . . . . . 380—388

The first six moments of χ^2 for an n-fold table with n degrees of freedom when some expectations are small. By J. B. S. HALDANE . . . . . 389—391

The approximate normalization of a class of frequency distributions. By J. B. S. HALDANE . . . . 392—404

MISCELLANEA:

(i) A note on a form of Tchebycheff's Theorem for two variables. By P. O. BERGE . . . . . 405—406

(ii) Note on J. B. S. Haldane's paper: "The exact value of the moments of the distribution of χ^2". By W. G. COCHRAN 407

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